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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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The Journal *of* Parapsychology

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EDITORIAL

THE RELATION BETWEEN PARAPSYCHOLOGY AND GENERAL PSYCHOLOGY

STRANGELY ENOUGH, in attempting to describe the relationship of parapsychology to general psychology we are handicapped most by the lack of a clear definition of psychology. There is probably more confusion over what is meant by the science of psychology than there is over any other branch of science. Among psychologists there is general disagreement, not only about the outer bounds of their field, but even about its more central or basic concepts. It is therefore not surprising that some psychologists have been unable to determine just where parapsychological findings legitimately belong with respect to psychology.

Although originally defined as the science of the psyche or soul (that is, of the mind as a distinct reality) psychology swung around about a generation ago to a quite contrary position, becoming, as the popular expression went, a science *without* a soul. The mind was either defined out of existence or was ignored as scientifically unapproachable because it was not directly observable. For a decade or two behaviorism became the dominant school of American psychology, with behavior, not the behaving person, as the object of study. Behavior, of course, is physical action, and if psychology could be defined as the science of behavior, presumably it would belong with the physical sciences.

More recently large numbers of psychologists have recognized the inaccuracy of such a definition of their field and have readmitted

mental phenomena to consideration, defining psychology as the science of behavior *and* experience. Most of them hasten to give assurance that no dualism is assumed by such a definition and that no distinction between mind and matter is necessary or is intended. Such a concept of the subject may not be very clear, but it has great vogue.

The latest contribution to the confusion is the currently favorite definition of psychology as the science of the relation between the organism and its environment. Such a formulation makes psychology unreservedly identical with the field of ecology. While this definition has the merit of extreme inclusiveness, it is really a retreat into obscurity; it as much as says that psychologists do not yet know the nature of their field.

It cannot be supposed that psychologists will long find this latest definition acceptable; it is hard to see how it could be a useful one. But, in the meantime, and for our present discussion, we need to have a working concept of psychology that is more to the point than this borrowed ecological one. To find such a working concept we need only ask ourselves what it is that psychologists study or work at when they are on the job. The obvious answer, of course, is that they study and work with people—with individual persons and with groups of them. Or if other than human species are studied, psychologically it is the personality-like properties or capacities of these that are of interest. It is not, therefore, behavior but the behaving person that is the subject of study; not the person's relation to environment merely, and not simply his experience, but himself as a person distinct from a nonpersonal thing. To be sure, behavior, experience, environment, and their interrelationships furnish the data, the raw material necessary in the study of people and in dealing with their problems. So far, then, the definition of psychology is obvious: It is simply the study of persons.

Here, however, is the rub: What is the difference between a person and a nonpersonal thing? A few of the great psychological leaders, such as McDougall and Stern, kept this question alive during their lifetime; but in the main, psychology has never seriously done much about it. Psychologists have therefore never known just where to place the person or personality in the scheme of nature. Many, like the behavorists, have gone so far as to rule out of court

the conscious experience they could not explain, dismissing by definition the difference between a person and his nonpersonal world.

The definition of psychology, however, is too important to be allowed to so shift from one misconception to another. For definition, of course, determines what belongs in the field and what the relations of psychology to other branches of science are. Obviously, then, the more confusion and insecurity psychologists feel as to the main outline of their field, the greater will be their hesitation to consider such radically new findings as parapsychology presents.

* * * * *

What now is parapsychology, and where does it fit in? Since there is no reasonable doubt that it deals with persons, it is obviously a branch of psychology, welcome or unwelcome as the fact may be. We can now indicate a fairly definite boundary for its problems and findings within the general area of psychology. All parapsychological phenomena are confinable within the single concept of nonphysicality; that is, they seem to defy physical explanation. Parapsychologists, lacking textbooks and classrooms, have not long or often been called upon to define their field of study, and there have been but few attempts to formulate definitions. We believe, however, that agreement as to this one will be fairly general among those who are more actively concerned with parapsychology.

A few examples will illustrate the concept. Suppose that one experiences the familiar voice of his brother calling for help at the instant the brother is drowning a thousand miles away. One thinks of this as a parapsychological or psychic experience because it could not be explained, as far as is known, in terms of physical principles. In such cases it is only when all possible explanations of communication by physical means can be satisfactorily eliminated that the experience is considered of interest to parapsychology. The same reasoning is applied to such an experience if only an intuitive impression impels one to say, "My brother has drowned," or if one has a symbolic dream of the tragedy.

Again we can illustrate with the claims regarding the lowly divining rod. If the reports of successful dowsing (for example, the location of underground water) are established as reliable and a physical explanation is found, the phenomenon will go without reservation to the physicists or engineers. If, on the other hand,

no physical explanation (of significant results obtained under adequately controlled conditions) seems possible, dowsing just as unquestionably becomes a parapsychological performance.

In the case of psychokinesis, even though the resultant effect is manifestly a physical one, there is the appearance of a defiance of physical law in the action of the mind on the moving objects in the PK tests. It is this significant feature of appearing in some way and degree to transcend physical law that characterizes the parapsychological in all its representations. *Parapsychology, then, is the study of those phenomena attributable to personal agency which in some degree transcend physical explanation.*

No one can say at this stage all that is likely to be included in parapsychology besides those psi phenomena that have already been tested with respect to the physical criteria of time, space and mass. It remains, of course, for the research of the future to discover how far the nonphysical feature of personality extends into what has been orthodox psychology. It is quite conceivable that eventually the major part of the whole subjective area of human experience might qualify as belonging to parapsychology as the latter is now defined. Such an outcome appears rather likely. However, in the revolution of psychological thought that we think must follow the acceptance of the findings of parapsychology, the nonphysical nature of its phenomena will become part of a new orthodoxy, and the need for the prefix "para" will disappear. But it is the present with which we are now concerned.

* * * * *

Not all the barriers between parapsychology and general psychology, however, are raised by trouble with definitions. Merely defining parapsychology as a subdivision of psychology will not in itself suffice to establish it there in actual practice, although clearing up this point should eventually make for better relations. There are other types of misunderstanding that create barriers between the two fields and these also urgently need clarification.

One of the most effective bars to recognition of psi by psychologists is the bare fact of the inexplicability of the psi phenomena. ESP, PK, and especially precognition, standing as strange, isolated, unclassifiable phenomena in nature, meet with slow acceptance from those cautious conservatives who must have an explanation of things

as a prerequisite to acceptance. For them, some explanation, even if only a plausible but untested hypothesis, would provide an acceptable passport for these foreign concepts. Such psychologists are not really explorers, and there would in fact be no science if a full explanation of each new discovery had to come along with the discoveries. Yet the very misfit character of psi phenomena that challenges our present theoretical system and thus makes them appeal to the exploring scientist as especially significant has much to do with the cold reception parapsychology has received.

All the student of psychology needs to do, however, to correct his perspective with regard to parapsychology is to recall how little is really known about even the most common processes of mental life. The more elemental phenomena of the human mind are hardly any better explained and understood than these very elusive psi capacities. They are merely more *familiar*. If one is puzzled, as he should be, about how the subject can interact with the object in ESP and PK, he needs only to ask either the neurologist or the psychologist how thought interoperates with the brain and he encounters about the same order of ignorance as that which embarrasses the parapsychologist. The well-informed student of psychology realizes that the whole realm of conscious thought process—consciousness itself—is just about as mysterious as psi.

In any case the alert scientist is eagerly seeking every odd fact that is relevant to his field in the hope of finding new clues to the solution of his problems; and, it is true, a number of psychologists, more especially the younger men, are turning to the work with psi processes in that spirit.

Beyond question, the greatest barrier to the acceptance of psi phenomena by psychologists has been the naïve fear of mind-body dualism that prevails. There has been an almost unreasoning readiness on the part of the intellectually insecure psychologist to believe in even the crudest attempt at the unification of the world of experience with the realm of behavior (witness behaviorism!), rather than have the uncertainty of an unresolvable dualism on his hands. But his apprehension is quite misplaced in this instance. The findings of parapsychology do not at all change the rational need for integrating our knowledge of all interacting realities. What these

psi findings do signify is extremely important, but it is distinctly not dualism as the term is used.

Rather, these discoveries regarding the nonphysical nature of psi furnish evidence of a greater diversity *within* the total personality of man than has been known or admitted by the psychological sciences. Now we have to speak of body *and* mind as component parts of the whole. *Thus without in any way altering the necessity for a comprehensive and unitary explanation of human personality as a whole, the psi researches do show plainly that there are mind-matter differences within the individual person more profound than anything else known in nature,* even though at the moment they can be defined only in terms of physicality and nonphysicality. Such differences may greatly complicate the task of discovering the common basis of interaction between physical brain and mental experience; but they do not alter in the least the fact of interaction. This fact is in any case the only basis we have for the concept of unity.

To understand this point would be to take much of the sting out of psi for the average psychologist. Thereafter his aversion to dualism would be no longer activated by the mention of parapsychology. He would realize then that what parapsychology does to the general field is to *broaden* it and *diversify* it, but not *divide* it. Even while it marks psychology off as no mere dependency of physics, it makes it no less natural—not a whit less scientific. Instead, to change the figure, parapsychology contributes to psychology the keystone long needed for its conceptual arch; namely, a scientifically demonstrable difference between persons and things, in the fact that things do not transcend physical principles, while persons do, at least in their psi operations. This is not the whole of the difference, certainly; but it is the only reliable one yet found that stands objective experimental test.

Hence, the psychologist may now rightly claim a degree of security regarding his field that is wholly new in its history and he may properly experience an independence that has not hitherto been known in the profession. He may now know with assurance that the universe is not, as he had drifted into taking for granted, wholly physical, and the part that is extraphysical is his special domain to explore and possess by proper scientific conquest. New world though

it be, however, it is as much a part of the universal sphere of psychophysical causation as the western hemisphere is a part of the globe.

* * * * *

We may hopefully regard it as only a matter of time until parapsychology is fully integrated with general psychology. Any other development is quite unthinkable if we are to count on progress. The amount of time required may be left an open question; it will largely be up to the psychologists.

What in general may we reasonably expect from a close rapprochement of the two fields? There is space here for only a few suggestions, and these have to do with the effects of the union upon psychology rather than parapsychology; we do not imply, of course, that the effects will all be one-sided. There have already been advantages to the psi studies from the field of psychology proper, as for example in the use of some of the methods of personality measurement. But the consequences for psychology of adopting the parapsychological findings appear likely to be radical and far-reaching indeed.

One of the main consequences will be that psychology can then quit fumbling with its fundamental problem of volitional freedom. Although almost the entire population of the world adheres to the concept of free will as a universal faith, psychology has hitherto had nothing to contribute on this important issue. The concept seems to be interwoven into our whole cultural structure; but is it true? Certainly if man, together with his environment, constitutes a unitary *physical* system, there would be nothing in him that could be considered free in any real way from anything else. However, since personality is found in the psi experiments to have certain properties that transcend physical law, and since it shows evidence of principles having a different order of lawfulness, the subjective self can understandably operate with some measure of true freedom from the objective order in which it lives (as a clutch may be let in or out to integrate or separate two different mechanical systems). Parapsychology thus finds scientific confirmation for one of the most fundamental commonsense principles in human conduct, that of the freedom of the will.

Having a case made for volitional freedom, the psychologist may

now take hold of the study of the various social freedoms and political liberties in all their many ramifications. As it is, they take much of their own significance from the volitional freedom exercised by the individual person. In the light of the psi researches, man appears more like the complex person his intuition has pictured him through the ages to be, one in which the self, operating on one set of principles, enjoys some independence of operation because of the difference in its laws from those of the rest of the organism and its environment. This can be only a partial freedom, it is true, because it is limited by interaction itself, which is binding and unifying. But it is enough for the liberation of the individual from the concept of determinism which materialism has projected into our Western culture. Now, too, with freedom of choice established as a possibility by the psi research, psychology should have much to offer in the development of a science of personal responsibility, or ethics. What could it ever have done on that great project without freedom of choice?

Still further, psychology may now go on to realign itself in full with the culture to which it belongs, but with which, for more than a century, it has been increasingly at odds. In all the principal social institutions man is still regarded traditionally as a psychocentric being, in which the self or mental system is central. Unhappily, psychology let the mind drop out of its scheme of things somewhere along the way because minds awkwardly refused to fit in with the chart of the universe then followed. The thought (or verbalization) of having to deal with a mind *and* a body, in a day when all the other sciences had united their separate phenomena into one grand universe, was too much for the diffident young science psychology then was. Today, however, parapsychology submits evidence to show psychology that it should never have let go its claim to the domain of a transcendent mental reality. More than that, it demonstrates that nonphysical processes may be studied by the objective methods of science and need not be considered as supernatural and impossible of integration with the physical world. Indeed, if they interact with the physical realm, there is some basis of integration; it only awaits discovery.

Finally, after centuries of talk about the energies of men, about psychical forces and the power of the will, psychology should be

able to attack the question of the nature of psychical energy in a controlled experimental manner. From the psi investigations we have evidence of causal action comparable to that which led to the inference of other energies in the past. What psychology has lacked (in the past) has been a clean-cut quantitative approach that would permit the demonstration of energetic effects under conditions in which all the known energies are either under control or else are eliminated from the experimental situation. The psi researches have afforded the long needed grip, psychology's first experimental grip, on this problem of the quantitative study of psychical energy.

It is no exaggeration to say that problems of novel character may be expected to spring up like seedlings from every fruit dropped in the further advance of parapsychological inquiry. On the one hand, the more we can find out concerning the place of psi phenomena in the structure of personality, the more we are likely to need to reorganize our conceptions of personality structure. On the other hand, what we shall find regarding the localization in the brain of the various functions that operate in psi phenomena should give us new alignments for the study of the great psychophysical gap that, more than anything else, has baffled man's understanding of his own fundamental nature.

All these consequences for psychology of accepting the findings of parapsychology are necessarily vague. Yet they are so sweeping that perhaps a generation of research could well be involved in each of them, or many generations if our researches have to compete with the continuing distractions of impending global crises. It is, however, rewarding to the humblest worker, to one who pushes back the curtain of ignorance ever so little, to realize something of the magnitude of possibilities the discoveries in parapsychology seem to hold for the whole broad range of psychological studies and their applications.

Unimpressive as parapsychology, with its humble beginnings in simple tests and tedious statistics, may seem to the onlooker, its fast approaching adulthood may be expected to be vastly different. And general psychology, the science to which manifestly belong man's greatest problems, should be expected eventually to assume the leadership of the sciences; but in order to do this it must adopt the

orientation to which parapsychology points it and take over the domain of distinctive mental reality as its proper base of operation. If,—or better, *when*—parapsychology succeeds in leading its parent science to realize the magnitude of its mission and responsibility in human life, the question of relations between the two sciences need give us no further concern since they will no longer be two.

J. B. R.

CHANGE OF CALL IN ESP TESTS

By J. G. Pratt

ABSTRACT: The investigation reported here by Dr. Pratt was carried out in order to test a hypothesis put forth by Dr. S. G. Soal. The latter, studying the ESP card-calling records of his subject, B.S., discovered that B.S. appeared to call a different ESP symbol (that is, avoid repetition of the same symbol) more frequently on the next trial after he had just hit the target than after he had just missed it. Since the subject was not told his score until the end of the run, Soal offered the hypothesis that B.S. must have been reacting to a subconscious awareness of success on the target.

In order to test this hypothesis Dr. Pratt has examined the records of 25 other ESP subjects. Among the latter he found two who showed a statistically significant tendency to change their calls more frequently after hits than after misses, and he found a group of blind subjects who did the reverse. That is, they changed calls more frequently after misses than after hits. (The change-of-call evidence in these three new groups of data was still statistically significant after allowance had been made for failure to find the effect in all the rest of the records surveyed.)

Further analyses bearing upon the interpretation of these findings indicated that the evidence of change of call in the B.S. data did not support the Soal hypothesis. For the other cases that showed significant change of call, however, the findings indicated a reliable differential reaction to ESP hits and misses. The question of whether this reaction was a response to ESP success is left open pending the outcome of further research.—Ed.

Introduction

Research workers have long been interested in the question of whether the ESP subject can in any reliable way distinguish his hits from his misses. It would be difficult to overestimate the importance of the discovery of a way in which the subject can or does react directly to success in an ESP test. Conceivably this might enable us to bring the psi function under conscious control and thus remove what has been termed "the source of the difficulties in parapsychology" (9).

A finding presented by S. G. Soal (11) in the literature five years ago was interpreted by him as showing that the subject in one of his experiments was reacting reliably to ESP success. The evidence for this reaction was in the pattern of the subject's sequence of calls. The subject showed a pattern of symbol calling after a hit that was different from that occurring after a miss. After call-

ing a symbol that scored a hit, he tended on the next trial to call a different symbol. That is, a call that made a hit was less likely to be repeated on the next trial than was one that missed the target. Dr. Soal offered the hypothesis that this difference in calling indicated that the subject was subconsciously "aware" of having made a hit. This difference between the call sequences after hits and after misses will be referred to as the "change of call" effect, a descriptive term which is used in this paper without implying any explanation.

The present paper is a preliminary report on research that was undertaken as a result of Soal's finding. The ESP records of a number of high-scoring subjects were examined to see whether change of call in relation to success and failure is present to a statistically significant degree. Such an effect was found, and the results were examined more closely to see whether they supported the hypothesis of unconscious awareness of success offered by Soal. The question of how to interpret the results has proved to involve many complications of which we have not yet seen the end.[1]

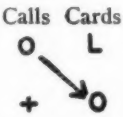
Previous Evidence of Change of Call

The work of Soal mentioned above as having produced evidence of change of call was his study in 1944 of a part of the data of his subject, B.S. This subject exhibited the peculiarity of scoring above chance on the target card that was one step ahead of the card at which he was supposed to be aiming his call. This form of scoring is known as forward displacement. In the tests in questions B.S. also made high scores on the cards that came just before the cards at which he was supposed to be aiming (backward displacement), but the only high scoring of B.S. with which we shall be concerned in this paper is his forward displacement.[2] At the same time that

[1] I wish to express my appreciation to the many people who have contributed to this research. My special thanks go to Mrs. Esther B. Foster, Research Assistant in the Parapsychology Laboratory, for her active participation in the work reported; to Dr. Dorothy Martin and Dr. Frances Stribic of the University of Colorado for the use of their data; and to both Dr. T. N. E. Greville of the Federal Security Agency and Dr. Herbert Robbins of the Institute of Statistics, University of North Carolina, for their statistical counsel.

[2] In some tests B.S. made his calls at a "rapid" rate, about twice as fast as normally, and the forward displacement scoring was on the second card ahead. In these records Soal studied the change-of-call effect by comparing the call scoring a forward displacement hit with the second following call. He pooled the records for the normal rate and rapid rate trials.

he was scoring on displacement, he was scoring significantly below "chance" on the direct target. Soal was led, therefore, to look for change of call after hits on forward displacement as a possible explanation of the negative score on the direct target. If such a tendency was present, it would produce the negative deviation in question, for each time a forward displacement hit was followed by a change of call on the next trial, the subject would automatically miss the direct card. For example:



Specifically, Soal's study was based upon an analysis of the subject's sequence of calling in four possible categories: (1) a forward displacement hit followed by a change of call; (2) a forward displacement hit followed by the same call; (3) a forward displacement miss followed by a change of call; and (4) a forward displacement miss followed by the same call.

In his study, each run of 25 trials provided 24 pairs of calls to be classified in terms of these four categories (23 pairs in the rapid rate tests). When the pairs of B.S.'s calls had been tabulated in terms of these four groups, the results were presented in a fourfold table showing how frequently hits and misses were followed by a change of symbol or by no change on the next call. To ascertain whether the distribution was reasonably explainable by chance, Soal applied a chi-square test to these results. His table and evaluation are given as Table 1 of this report. The upper figure in each cell shows the observed number of cases; the lower figure (in parentheses) shows the most probable number if there was only a chance relationship among the four kinds of pairs of calls. Soal found that, relatively speaking, there was a highly significant tendency for hits to be followed by change of call. The chi-square value obtained has a probability of .0002, which is very significant. It shows that B.S. undoubtedly had a statistically reliable tendency to change his call after making a hit on forward displacement.

Later in this paper Soal's interpretation—that this finding suggests that the subject was subconsciously reacting to success—will be examined to see whether it is justified. At this point we may

Table 1

CHANGE OF CALL FOLLOWING FORWARD DISPLACEMENT
HITS AND MISSES IN THE B.S. DATA OF 1942-43

Second Call of Pair	First Call of Pair: Hit	First Call of Pair: Miss	Total
Change	425 (399)	936 (962)	1361
No Change	54 (80)	218 (192)	272
Total	479	1154	1633

$\chi^2 = 13.6$
$P = .0002$

merely observe that the interpretation, if it is the correct one, provides the clue that the research workers have been needing so desperately, the clue that the subject is somehow aware of the difference between success and failure. As Soal suggests, this interpretation does not imply conscious awareness, but only such discrimination between hitting and missing the target as would be necessary to produce the observed difference in response after success and failure.

NEW EVIDENCE OF CHANGE OF CALL

Although B.S. did his positive scoring in the form of displacement, there seemed to be no reason why the psychological effect of ESP success would be unique for that sort of scoring. I therefore began to look for the change-of-call effect in the records of subjects who scored significantly on the target at which they were aiming. It seemed best to begin by finding out whether change of call does occur to an extrachance degree and then to make such further analyses as might be required to find out whether it is a psychological reaction rather than a statistical artifact. If change of call as a psychological reaction does occur, it is reasonable to expect that in some subjects it might appear as a tendency to change the call symbol more frequently after misses than after hits.

To avoid the confusion that might arise from the fact that B.S. scored high on forward displacement while all the other subjects scored on the direct target, we will refer to the card on which the

subject made his highest score as the *main target*. We will be concerned, in other words, with whether any particular call symbol was a hit or a miss on the main target and whether the next call symbol was changed or not changed.

As the first case to be examined I selected the subject D.W. from the Preliminary Series of the extensive University of Colorado experiments (4) because I had the data conveniently on hand from another study just made. (In this work, as in all of the records to be examined, the subject was not told of her success until the run of 25 trials was completed. There was therefore no possibility that the pattern of calling within the run was influenced by knowledge acquired through sensory channels.) The D.W. series consists of 301 runs of tests for clairvoyance using the "down through" (DT) procedure. Since each run of 25 trials offers an opportunity of observing 24 pairs of calls, there was a total of 7,224 observations for the series. Each pair of successive calls was tabulated according to the four categories mentioned earlier; that is, the pairs were first separated as to whether the first call was a hit or a miss on the main target and then as to whether the second call was a "change" or "no change" from the first. The results, as given in Table 2, show highly significant change of call after hits.

Table 2

D.W. Data on Change of Call

Second Call of Pair	First Call of Pair: Hit	First Call of Pair: Miss	Total
Change	1666 (1597)	3757 (3826)	5423
No Change	461 (530)	1340 (1271)	1801
Total	2127	5097	7224

$\chi^2 = 17.09$

$P = .000,04$

This analysis gives a chi square of 17.09, which, with one degree of freedom, has a probability of .000,04. From this result it is clear that the D.W. data show the effect of change of call after hits.

At the time of preparation of this report the analysis has been extended to cover the data of 25 subjects.[3] Among the series that have been studied, two have given statistically significant results. One was the work of C.J. in the Main DT Series of the University of Colorado experiments. (Only 900 of the 1,000 runs of the C.J. series were available for analysis.) The other was the work of one subject from the experiment with blind children in the North Carolina School for the Blind in Raleigh by Price and Pegram (6). Eleven subjects in this series who were outstanding for the number of runs done and for score average were selected for this investigation. The records studied were obtained under a variety of standard ESP test conditions in which a record of the call sequence was kept. The detailed results of the change-of-call analysis of the C.J. data are given in Table 3 and those for the Raleigh blind subject in Table 4.

Table 3

C.J. Data on Change of Call (Main DT Series)

Second Call of Pair	First Call of Pair: Hit	First Call of Pair: Miss	Total
Change	5037 (4935)	13032 (13134)	18069
No Change	862 (964)	2669 (2567)	3531
Total	5899	15701	21600

$\chi^2 = 17.86$

$P = .000{,}02$

[3] The subjects to be analyzed were selected chiefly on the basis of their having made highly significant ESP scores over a sufficient number of runs to make a study of change of call feasible. In the case of one subject, Woodruff, only chance scores were made in the series analyzed; these data were included because the earlier records, in which this subject had scored significantly, were not available, and it was deemed worth while to see if an outstanding subject might show change of call even when he was no longer scoring above "chance." None of the data were examined in any way for change of call before the decision to study them was reached, so there was no opportunity for any kind of favorable selection of series. If a subject showed no evidence of change of call in the first groups of his runs studied, the analysis was sometimes stopped for that particular subject. However, Table 5 includes all of the results of all the runs analyzed (up to the time the decision was made to stop in order to write this preliminary report).

Table 4

CHANGE OF CALL IN THE DATA OF A BLIND CHILD SUBJECT (D.F.)

Second Call of Pair	First Call of Pair: Hit	First Call of Pair: Miss	Total
Change	203 (209)	676 (670)	879
No Change	14 (8)	19 (25)	23
Total	217	695	913

$\chi^2 = 6.55$
$P = .010$

The results of C.J., like those of D.W., show a tendency to change the call more frequently after hits than after misses. The chi-square value obtained is 17.86, which has a probability of .000,02. The results for the Raleigh blind subject are also significant ($\chi^2 = 6.55$; $P = .010$). In the data of the blind child subject the tendency was to change the call symbol relatively more frequently after *misses* than after *hits*. In fact, for the total data of eleven subjects studied in the Raleigh series there was a fairly general tendency to change after misses. When the results of the entire group are pooled, this effect gives a chi square of 8.05 ($P = .007$).

The results from all of the series analyzed in the present investigation, including those subjects already presented in detail, are summarized in Table 5. This table does not, however, include Soal's results. The statistical significance of the total results may be obtained by combining the individual probabilities by Fisher's method. The combined probability is .00042, so the effect as a statistical finding is still significant when considered against the background of all the data surveyed.

In the effort to throw further light upon the nature of the effect we may now profitably confine our attention to the subjects who contributed to the statistical significance shown in this first general analysis. We shall deal in the remainder of the present paper, therefore, with the results of D.W., C.J., and the Raleigh blind subjects. The Raleigh blind data will be treated as a group in order to get

Table 5

SUMMARY OF EVIDENCE FOR CHANGE OF CALL IN ALL THE ESP RECORDS CHECKED IN THE PRESENT INVESTIGATION

Subject	Experiment	Runs	Change of Call	
			χ^2	P
D. W.	Martin & Stribic (4)	301	17.09	.000036
C. J.	Martin & Stribic (4): Main DT Series	900	17.65	.000022
D.F.	Price & Pegram (6)	38	6.55	.010
J.Ma.	Price & Pegram (6)	35	5.04	.025
J.T.M.	Price & Pegram (6)	30	4.03	.044
E.G.	Price & Pegram (6)	47	1.84	.17
Ri.M.	Price & Pegram (6)	36	1.51	.22
T.A.	Price & Pegram (6)	58	1.34	.25
A.H.	Price & Pegram (6)	20	1.32	.25
H.R.	Price & Pegram (6)	21	1.17	.28
E.R.	Price & Pegram (6)	40	1.02	.31
Pat.N.	Price & Pegram (6)	38	.80	.37
J.H.	Price & Pegram (6)	23	.08	.98
Riess's	Riess (10): Series A	74	.26	.61
Pearce	Rhine (8): Pearce-Pratt Series	74	.00	1.00
Pearce	Rhine (7): Regular BT & DT data	322	.39	.54
L.H.G.	Gibson(2): BT & GESP data	304	.19	.66
G.	Carpenter & Phalen (1): Series 7	100	.06	.98
L.	Carpenter & Phalen (1): Series 1	200	.04	.98
M.	Carpenter & Phalen (1): Series 1 & 2	230	.00	1.00
Pegram	Pegram (5)	362	.18	.67
Sisler	MacFarland (3)	90	.71	.40
Van Horn	MacFarland (3)	111	.14	.71
Estes	MacFarland (3)	47	.06	.98
Woodruff	Woodruff (12)	396	.35	.56

Combined P (Fisher's Method) = .00042

enough data for the more detailed analyses to be made. It seems legitimate to do this, as they were fairly consistent as a group in showing the *inverse* change-of-call effect, or a larger proportion of changes of call after misses than after hits.

INTERPRETATION

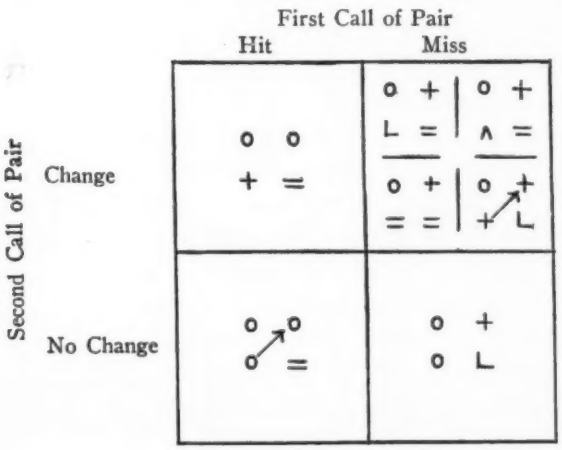
Is the change-of-call effect really a reliable psychological reaction to ESP success and failure as such, as Soal's hypothesis implies? The first step toward answering this question is to see whether there is any way in which change of call can be explained without relating it *specifically* to hits and misses on the main ESP target. In other words, is it possible to explain the findings as arising from a purely statistical effect? Even if the answer to this question is in the nega-

tive, it is still conceivable that change of call may be related in a direct or trial-by-trial way to hits and misses on the main target without indicating any kind of reaction to success or failure. If it proves to be, at least in part, a psychological effect specifically related to the main target we will still be left with the task of finding out what the effect means. The present paper will deal only with the more general question of whether the results indicate *some kind* of response specifically related to hits and misses on the main target.

Generally Dispersed ESP

One possible explanation of the change-of-call effect is that the subject's ESP is operating in more than one way, as when it shows up in the form of forward or backward displacement as well as in the form of scoring on the main target. Consider the case of a subject like D.W. who was scoring well above "chance" on the card at which she was aiming her calls (main target). If, at the same time, her calls were scoring below "chance" on the card coming just before the main target (backward displacement), with the two kinds of scoring occurring independently of one another, her records would have shown the change-of-call effect as a purely statistical effect produced by this dispersed scoring. In the same way, if the Raleigh blind children who scored above "chance" on the main target had also scored positively on backward displacement, that fact alone would have explained their *inverse* change-of-call effect.

How is it possible for generally dispersed ESP scoring to pro-



duce change of call as a statistical effect? The answer to this question is illustrated in the following fourfold table which shows the influence of backward displacement hits upon the distribution of pairs of calls in the four cells of the table. The two symbols on the left in each group of four symbols represent the subject's calls; the two on the right, the target cards. The diagonal arrows show backward displacement hits.

Suppose that the actual figures on pairs of calls that go to make up such a table for a change-of-call analysis are based on the records of a subject who was scoring positively on the main target and negatively on backward displacement. Each hit on the main target on the first call of a pair that was followed by a miss on backward displacement on the second call of the pair would fall in the Hit-Change or upper left cell, as shown. In other words, each time the subject succeeded in hitting the main target he would have to *change his call* on the next trial in order to miss on backward displacement. Following a miss on the main target, however, the subject can still miss the backward displacement target *without changing his call.* (See the example in the lower right cell.) There are, of course, also three ways in which he can change his call and still miss the backward displacement card, as shown by the first three examples in the upper right cell. To summarize: a miss on backward displacement after a hit on the direct target always requires a change of call; a miss on backward displacement after a miss on the direct target does not do so. Because of these facts change of call would appear in the records of our hypothetical subject as a statistical artifact. The change-of-call analysis would simply be another way in which the dispersed ESP would show up.

In the same way it may be seen that positive scoring on both the direct target and on backward displacement would produce the opposite effect, or change of call after misses.

It is necessary to examine the data of each subject who showed the change-of-call effect to see if the results can be explained on the hypothesis of dispersed ESP. If change of call is a psychological reaction that is related in a specific way to hits on the main target, those calls following misses on the main target should show no evidence of significant scoring on backward displacement. Or if significant backward displacement after misses is found but the score

deviates from "chance" in the opposite direction to that required to account for the observed change-of-call effect, the hypothesis of generally dispersed ESP could not be offered as the explanation.

How can backward displacement after misses be evaluated in a way that is not complicated by the subject's individual tendency to change or not to change his call? Call symbols that *miss* on the target at which the subject is aiming and that are *repeated* are automatically also misses on backward displacement. For these calls it is impossible to distinguish between the two effects and therefore these trials cannot be considered when we are looking for backward displacement as a generally dispersed form of ESP. However, if backward displacement scoring occurred as a *general* effect, it would have to show up when the call symbols were changed after misses, as represented by the second calls of the pairs shown in the upper right cell of the illustrative table above. If we do not find negative backward displacement there we may safely conclude that there was no generally dispersed ESP to account for change of call.

In the case of B.S.'s 1942-43 work analyzed by Soal for change of call, the hypothesis of generally dispersed ESP is adequate to explain the change-of-call effect. The positive scoring, we recall, occurred on forward displacement, and the question was how to explain the negative scoring on the direct card. Soal thought that the finding of change of call after *hits* on the main (forward displacement) target largely explained the negative scoring on the direct target, as each such change automatically gave a miss on the direct card. It was on the strength of this finding that he suggested the interpretation that change of call might indicate that the subject had an unconscious awareness of ESP success. Before this interpretation can be accepted it must be shown that change of call produced the negative scoring instead of the explanation being the other way around.

Soal's hypothesis is immediately called into question by the fact that he found that the negative scoring also occurred after *misses* on the main target. In evaluating the significance of the negative direct card score after misses on forward displacement, Soal took as the expected direct-card score one-fifth the total number of such misses. He found that those calls following misses on the main target gave a score of 201 hits on the direct target. By his method

of figuring expectation this total score represented a negative deviation with a CR of approximately 2.2.

The purpose of the above analysis presumably was to evaluate the negative score on the direct target following misses on forward displacement when the observed amount of change of call was taken into account. If this was indeed the objective, the Miss-No Change cases should not have been included, as there was no possibility that those calls would hit on the direct target. The analysis proposed in this paper, taking *one-fourth* of the number of cases in the Miss-Change cell of the fourfold table as the expectation, would have been more appropriate. I therefore re-evaluated B.S.'s negative score on the direct target for those trials on which he changed his call symbol after a miss on forward displacement. In view of what has just been said, we know that all 201 hits on the direct target which were made just after misses on forward displacement came in the upper right or Miss-Change cell. As one-fourth of the 936 changed calls observed (see Table 1) would be expected to yield hits on the direct target, the expected score is 234, and the deviation is -33. The SD is, of course, given by the usual formula, $\sqrt{npq}$, which becomes in this instance $\sqrt{936 \times \frac{1}{4} \times \frac{3}{4}} = 13.25$. The negative score on the direct target, when the subject's individual tendency to change his call in the data as a whole is taken into account, has a CR of 2.49, a significant rate of negative scoring after *misses*.

To explain the negative scoring in this part of the data, Soal brought in a second hypothesis, one quite distinct from that offered to explain the negative scoring after hits. The nature of this second hypothesis is of no concern here, but the use of *two* hypotheses to explain negative scoring on the direct target seems unnecessary and unjustified in this case. It is much simpler to say that the ESP was generally dispersed in these data and that the negative scoring on the direct target was only one form in which it appeared. At least, a further study of the data would be required before they could be said to offer any support for Soal's suggested interpretation.

In summary, since B.S. scored negatively on the direct target following misses as well as hits on the main target, the B.S. change-of-call tendency cannot be interpreted at this time as evidence of

some kind of subconscious knowledge of success. In fact, not only do the results not lend support to this hypothesis, but they also fail to indicate that there was a specific connection of any kind between change of call and the subject's scoring of hits and misses on the main target. The hypothesis of generally dispersed ESP seems fully adequate to account for change of call in this series as a purely statistical effect.

The next step was to find out whether the dispersed ESP hypothesis was also applicable to the two Colorado subjects who gave significant change-of-call evidence and to the Raleigh blind subjects who, as a group, gave significant results for the reverse effect, that of *repeating* their calls after hits. Since in all of these series the main target was the direct card, the backward displacement target is the one that must be examined for generally dispersed ESP. In line with what has previously been stated, change of call after hits is in itself certain to produce negative scoring on backward displacement in that part of the data. For all practical purposes, therefore, the two effects (change of call and negative backward displacement) are identical for the calls following hits. If a negative score on backward displacement occurs only after hits on the target at which the subject was aiming, this would appear to indicate some kind of psychological reaction specifically related to hits and misses on the main target.

As in the case of B.S., it was necessary to find out whether such dispersed negative ESP was present *as a general effect instead of occurring only on those calls following hits.* The only way to decide this was to find out whether a significantly low score was made on backward displacement when the subject changed his call after a *miss.*

By this criterion, D.W. was found to have scored at "chance" on backward displacement following misses; that is, when dispersed ESP is *considered as a general effect.* On the 3,757 trials when she changed her call after misses on the main target (see Table 2) she made a total score of 939 hits on backward displacement. The expected score is one-fourth of 3,757 or 939.25 hits. She was obviously not scoring below chance on backward displacement in general. In other words, negative scoring on backward displacement was not taking place independently of hits and misses on the main

target. In her case, therefore, the hypothesis of generally dispersed ESP will not explain the change-of-call findings.

The same analysis was applied to the 900 runs of C.J. data, and the results rule out this explanation just as definitely in his case. On the 13,032 trials when he changed his call after misses (see Table 3), his score on backward displacement was 3,351 hits. The most probable score by chance is one-fourth of the number of changed calls, or 3,258. Thus we find that the subject actually scored *more* hits on backward displacement for these calls than the number expected. The deviation of +96 does not represent a significant rate of scoring (CR = 1.94; P = .06). It is interesting to observe, however, that the deviation is actually in the direction opposite to that necessary to explain change of call in the C.J. data as a statistical effect arising out of dispersed ESP.

In the case of the Raleigh blind subjects, since the change-of-call effect was the inverse one, that of changing the call more frequently after misses than after hits, we need to see if the finding can be explained in terms of generalized *positive* scoring on backward displacement. For these subjects combined, there were 6,493 pairs of trials in which they changed their calls after misses. The backward displacement score within these "Miss-Change" pairs was 1,584, whereas the expected score was 1,623.25. This gives a deviation of −39.25 on backward displacement as a general effect independent of change of call. While this result is not significant (CR = 1.12), it is, as in the case of backward displacement for C.J., in the direction opposite to that necessary to explain the observed change-of-call effect.

To review briefly the findings of this section of the paper: The statistically significant change-of-call effect in Soal's subject, B.S., and in the three series discovered in the present investigation were examined to see if the results were explainable in terms of the hypothesis of generally dispersed ESP. This hypothesis was found to be adequate to account for change of call in the records of B.S., but not in the data of the other three series.

Variation in Calling Doubles Correlated with ESP Scoring

Another possible explanation of change of call that would not make it necessary to conclude that the effect is directly related to

ESP hits and misses must now be examined. This is the hypothesis than an individual subject whose records reveal evidence of change of call may have shown a systematic variation from session to session or from run to run in his tendency to call doubles, and that the number of doubles called was correlated with the ESP score. A negative correlation would produce the appearance of change of call after hits; a positive correlation would have the reverse effect. It is conceivable that a subject's success in demonstrating his ESP ability in the card test situation varies from session to session or from run to run and that his tendency to call doubles varies along with his general ability to score well by ESP. To account for change of call after hits by this hypothesis, it is necessary only to suppose that the subject calls very few doubles when he is scoring well and a lot of them when he is scoring poorly. This would produce the change-of-call effect on the basis of a general shift in the subject's manner of responding rather than as a differential trial-to-trial response related in a specific way to ESP hits and misses on the main target.

If there exists such a systematic relationship between the level of ESP scoring and the number of doubles called, we would expect to find a negative correlation between the ESP run score and the number of doubles called in each run. The B.S. data were not available for this correlation analysis, but as his evidence of change of call has already been shown to be explainable on the hypothesis of generally dispersed ESP, it need not be analyzed on any further basis. In the D.W. data, the correlation between the 301 run scores and the number of doubles called in each run was found to be +.02. The same correlation from the 900 runs by C.J. was found to be +.06. Both of these are insignificant values, but it is interesting to observe that the direction of the correlation in each instance is opposite to that expected if the change of call evidence were to be explained by this counterhypothesis.

Likewise there is no evidence from the Raleigh blind work that there was any general connection between calling more or fewer doubles and scoring high or low by ESP. The correlation obtained between the run scores and number of doubles called from the 385 runs of that series was +.004.

The failure to find significant correlations does not prove con-

clusively that no general relationship exists between the level of ESP scoring and the number of doubles called. We can only say that if such a relationship exists its presence was not revealed by the method of analysis used. It is reasonable to suppose in this instance, however, that the correlation analysis would reveal the effect in question if it were actually present. The absence of correlations of any statistical significance is a strong indication that variations from run to run or from session to session in the tendency of a subject to call doubles cannot explain the change-of-call evidence.

General Strength of Tendency to Call Doubles

This section deals with a more general point than the one we have been considering. The question here is: Will any peculiarity of the subject in calling, in his data as a whole, a larger or a smaller number of doubles than would be expected in a random series account for the change-of-call effect? The evidence bearing upon this question may first be considered in a general way and then more specifically.

It is commonly observed that subjects call fewer than the expected number of doubles. The expected ratio in a random series, such as one obtained by taking the order of the digits one to five in Tippett's table of random numbers, is that one-fifth of the pairs will be doubles. Among the subjects who showed the tendency to change the call symbol following a hit, D.W. called, on the average, more than the chance number of doubles for a random series and C.J. called, on the average, fewer than expected. Both of these subjects, however, showed the same change-of-call tendency. It is evident, therefore, that the generalization cannot be made that the data of overdoublers will show change of call after hits while that of underdoublers will do the opposite.

It is possible, however, that a subject may be an underdoubler in general, but that he overdoubles on one or two symbols and that these alone account for change of call. In fact, more detailed analyses that lie beyond the scope of this paper indicate that this is the case. A full discussion of how this finding relates to change of call will be presented in a subsequent report. For the present, I shall be concerned only to analyze the data of the three significant change-of-call series in a way that will show whether the effect is

simply the result of a breaking-up of a habitual general tendency toward overdoubling or underdoubling when a genuine ESP hit involving a different symbol occurs.

The point at issue can perhaps best be understood if we consider an extreme case of overcalling one symbol. Suppose a subject calls circles on every trial except when he changes his call in order to score an ESP hit on one of the other four symbols. This subject would show a strong tendency to change his call after a hit, because after making a hit on any one of the other four symbols (or two or more such hits in succession) he would immediately switch back to calling circles. The only hits not followed by change of call, therefore, would be those that occurred, either by chance or by ESP, on circles. After misses, on the other hand, this hypothetical subject would repeat his preferred call symbol (circle) in every case except when he changed in order to make a hit on another symbol.

Now if change of call after hits is an artifact of overdoubling because the hits break up habitual sequences of calling, the tendency to change should be just as evident on the calls preceding the hits. In other words, we should find that the call preceding a hit is different from that on which the hit is made just as often as the call following a hit is different. In analyzing the data to see whether this was true, it seemed best to avoid the complication that arises from considering those cases in which two hits occurred in succession. A chi-square analysis based upon a fourfold table was therefore made to answer the following question: When all those pairs of successive calls which show one hit and one miss are considered, are the two call symbols equally likely to be the same (that is, to form a double) when the hit comes before the miss and when the miss comes before the hit?

The analysis was applied independently to the D.W. and the C.J. data and to the work from the Raleigh blind subjects as a group. The results are shown in Table 6. The chi-square values of both Colorado results are significant by the .02 probability criterion, and that from the Raleigh work barely misses being so. The relationships shown in the table may be interpreted as follows: For D.W. and C.J., if the first call symbol of a pair is a hit, the second call symbol tends to be different, whereas if the second call of the pair is a hit, the preceding call tends to be the same. The

Table 6

CHANGE OF CALL IN PAIRS OF CONSECUTIVE TRIALS CONSISTING OF A HIT AND A MISS

D.W. DATA

Call Making Hit

Call Making Miss	First Call of Pair	Second Call of Pair	Total
Different	1196 (1158)	1133 (1171)	2329
Same	334 (372)	415 (377)	749
Total	1530	1548	3078

$\chi^2 = 10.36$ $P = .0013$

C.J. DATA

Call Making Hit

Call Making Miss	First Call of Pair	Second Call of Pair	Total
Different	3504 (3461)	3374 (3417)	6878
Same	653 (696)	730 (687)	1383
Total	4157	4104	8261

$\chi^2 = 6.40$ $P = .011$

RALEIGH BLIND DATA

Call Making Hit

Call Making Miss	First Call of Pair	Second Call of Pair	Total
Different	1431 (1450)	1494 (1475)	2925
Same	173 (154)	137 (156)	310
Total	1604	1631	3235

$\chi^2 = 5.31$ $P = .021$

finding is just the reverse for the Raleigh blind series. These relationships are consistent with those found for the change-of-call effect in the data of each series as a whole. The analysis therefore indicates that the three series with an overall change-of-call effect still have the effect when we consider only this limited part of their data wherein peculiarities of overdoubling or underdoubling could not have influenced the results.

It is recognized that this analysis, like the others done in this study in an effort to throw light upon the meaning of change of call, is based only upon the results of the three selected series. For this reason it may be appropriate to consider this analysis as being of suggestive value only. All that is claimed in the present report is that this finding, together with the others presented, justifies a further study of the change-of-call effect in the effort to reach a more definite interpretation. In connection with the relation of the amount of doubling to the change-of-call effect, however, it is interesting to observe that all of the twenty-three series that failed to show any overall effect contained less than the expected number of doubles in the calls. Clearly, if underdoubling produced inverse change of call in these subjects like that found in the Raleigh series, there must have been present at the same time some sort of opposite tendency that obscured this statistical effect.

The indication is, therefore, that change of call in the three significant series studied cannot be explained completely by peculiarities in the amount of doubling of symbols in the call sequence. In the above analysis (Table 6) it is not possible to say whether the significant results are due to the fact that the subject changed his call *after* a hit or repeated his call in order to *make* a hit, or both. It does indicate, however, that we are not dealing wholly with a purely statistical effect, as otherwise the data would be distributed in a random manner in the right and left halves of the three fourfold tables.

Still another analysis of the relation between the number of doubles called and the tendency to change the call after a hit was made. This was a chi-square test of the relationship between the change-of-call tendency and the number of doubles in each run. For the purpose of this analysis, each run was tabulated in a fourfold table according to whether or not it alone showed the change-

of-call effect and also according to whether the number of doubles called was greater or less than the subject's general average. In this analysis, a run was considered as conforming to the change-of-call tendency if the calls coming after hits were changed a larger percentage of times than were the calls coming after misses. Runs which showed the same proportion of changes of call after misses as after hits were omitted from this analysis. Only the data of D.W. and C.J. were analyzed in this manner.[4]

Table 7

THE RELATIONSHIP BETWEEN THE CHANGE-OF-CALL TENDENCY IN EACH RUN AND THE NUMBER OF DOUBLES CALLED

D.W. DATA (290 RUNS)

	Doubles Called Per Run: Five or Fewer	Six or More	Total
Runs Showing Change of Call	74 (72)	108 (110)	182
Runs Not Showing Change of Call	41 (43)	67 (65)	108
Total	115	175	290

$\chi^2 = .21$
$P = .62$

C.J. DATA (571 RUNS)

	Doubles Called Per Run: Three or Fewer	Five or More	Total
Runs Showing Change of Call	179 (187)	171 (163)	350
Runs Not Showing Change of Call	127 (119)	97 (105)	224
Total	306	268	574

$\chi^2 = 1.69$
$P = .19$

[4] The Raleigh data were not suited to this analysis because of the amount of variation among individual subjects in the number of doubles called. The reason is that these subjects varied widely in the evidence shown for change of call (see Table 5). An analysis of this effect in relation to the number of doubles called

In the C.J. data a disproportionately large number of runs had exactly four doubles. As the average number of doubles per run for all his data was very close to four, the runs with four doubles were omitted from the analysis to provide a more clear-cut dichotomy. The average number of doubles per run called by D.W. was between five and six per run, and all of her runs could be included in her analysis.

The results of the analysis (Table 7) show that, as far as this method indicates, there was only a chance relation in each run between the number of doubles called and the change-of-call tendency.

Concluding Remarks

This investigation of the change-of-call effect, starting out as it did from the earlier findings by Soal, naturally developed along the same line of interest. The point of view with which the work started was that change of call might prove to be interpretable as an effect which success in making an ESP hit has upon the subject's next response. If all of the change-of-call effects should prove to be traceable to those call symbols that come *after* hits, the hypothesis that the results show an unconscious reaction of the subject to ESP success might be the simplest explanation of the findings.

The results of the analyses presented in this first paper show that none of the counterhypotheses considered will account in full for change of call in the three series that gave significant evidence for the effect. At the same time, the analyses do not establish the findings as evidence of a subconscious reaction to success. Rather, they show only that the effect indicates some kind of psychological reaction that has *a direct and specific relation to ESP hits and misses.*

But further analyses that lie beyond the scope of this first report and which go into the matter in much greater detail raise the question whether the statistical significance of the results is all to be attributed to the call that *follows* a hit. In part, the explanation may lie in an influence that a target that is about to be hit by its own call has upon the call symbol that *precedes* the trial on which the hit is made. Furthermore, the possibility that a target symbol that

for this group as a whole might therefore lead to an apparent relationship between the two which would be a spurious result of pooling the data of a number of subjects. There were not enough runs from each subject to apply this analysis to each one individually.

is *missed* by its own call may be related in a statistically significant way to the calls that precede and follow it is not to be overlooked. It now appears that the facts, when they have been fully mustered, will be interpretable in terms of a *spread of influence* about ESP targets, the direction of the influence about hits being opposite to that about misses. If these indications are borne out by the further investigations that are now being made, change of call may prove to be only a secondary effect arising out of these newly discovered peculiarities of the ESP process.

The facts that have been uncovered thus far in this investigation may well be important clues to the nature of the ESP process. They would appear to justify fully whatever further study may be necessary to find out what they mean.

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PSYCHIATRIC CONTRIBUTIONS TO PARAPSYCHOLOGY: A REVIEW

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Considering the potential significance of psi phenomena in any view of man's personality and behavior that aims at comprehensiveness, it may appear somewhat strange that psychiatrists have taken so little notice of and have made so few contributions to parapsychology. But this is looking at things from the side of the parapsychologist. From the psychiatric point of view there is every reason to be suspicious of a field of study which takes seriously a group of alleged phenomena and a set of propositions which correspond closely to delusions that have always characterized the mentally ill. These delusions, moreover—of thought transference, of influencing and being influenced by minds at a distance through means that transcend the recognized senses—invariably disappear as the mentally disturbed regain the capacities, the balance and the relationships to people and things that are generally accepted as normal conditions of mental health.

Psychiatry—particularly the dynamic psychiatry of Freud and his beneficiaries (if not always followers)—has always taken pride in its discovery of the origin of these delusions of the mentally ill. They are regarded as residues of a phase of infancy through which everyone passes, a phase characterized by "omnipotence of thought" and destined to be superseded by a less magic, less wishful type of thinking as the demands of "reality" force the maturing individual to adapt to the world "as it is." These residues, it is known, are latent in all of us, and may emerge to warp one's entire thinking if the mind under stress is forced to regress to infantile modes of functioning.

One may see, thus, why psychiatrists on the whole have been leery of claims that have, according to their teaching, the distinct taint of morbid, disturbed thinking. One might as well expect jurists and officers of the law to find the values and rationales of

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the underworld reasonable and perhaps worthy of serving as the basis of a new system of ethics.

Nevertheless, a number of psychiatrically trained observers have made contributions to the field of parapsychology, several of them of signal importance. It is the purpose of this paper to review this work. The intent of the reviewer, however, is not simply to list, outline or paraphrase every mention of psi phenomena or parapsychology made by psychiatrists in various literatures, but to describe those contributions which have been oriented toward unifying or at least bridging the gap between the subject matters of psychiatry and parapsychology. Thus, papers or statements by psychiatrists who have simply described psi phenomena much as any nonpsychiatric observer might have—that is, without using any special psychiatric technique to effect such observations or deriving therefrom no suggestive hypotheses relating to either field—will not be mentioned in this review. Nor will, for that matter, any number of oblique or direct statements of favorable opinion on the reality of psi phenomena or unsupported generalizations as to their nature made by psychiatrists, however eminent, who have had nothing else to offer. These would not be *psychiatric* contributions to parapsychology, but merely "contributions" by psychiatrists. On the other hand, there are instances of contributions by workers who are not psychiatrists but whose observations have been at once psychiatrically oriented and significant. These, of course, should be mentioned.

It is now somewhat disconcerting to find that an exception must immediately be made. I must begin this review with the observations of a psychiatrist who failed to bring his special training to bear in studying and reporting one of the most remarkable series of phenomena in the annals of parapsychology. But this work, even in its inadequacy from the psychiatric point of view, is highly instructive in that it illustrates, by implication at least, the tremendous resistance in a scientifically trained person to viewing psi phenomena with any sort of detachment. The psychiatrist was none other than Pierre Janet, and the phenomena he was called upon to witness were not such as had to be smoked out by patient and devious methods of investigation. They hit him broadside on.

In 1884 Janet, then a young psychiatrist of growing eminence, was invited to Havre to witness the experiments of a Dr. Gibert

with his subsequently famous subject, Léonie. Léonie was a French peasant woman whom Gibert claimed he could hypnotize at a distance in the same room—that is, without saying anything or doing anything perceptible, but merely *willing* the entrancement. Janet found that this was apparently so, and that he himself was able to do the same thing. Later, distances up to 500 metres were attempted before witnesses and under well-controlled conditions—for instance, the time for each trial being determined by lot by one of the observers. In two papers read in 1885 and 1886 before the Society of Physiological Psychology in Paris, the great French neurologist and psychiatrist Charcot presiding,[2] Janet reported on his and Gibert's work with Léonie (**28, 29**). In all, twenty-two distance experiments were attempted, of which six were failures and sixteen were successes *"précis et complet."* (All incomplete trials were counted as failures, as were also attempts when sleep—usually catalepsy—did not come on after fifteen minutes.) In certain of the trials posthypnotic suggestions mentally given (for example, taking a lamp into another room and lighting it in broad daylight) were successfully carried out. "Are we to believe," writes Janet (**30**), "that on sixteen occasions there was a rather exact coincidence? Such a supposition is a little unreasonable. Were there at any time involuntary suggestions on our part? All I can say—and I say this with utmost sincerity—is that we took every possible precaution to avoid this. We can conclude only one thing: that such phenomena should be reproduced and studied."

The point of the story, however, is that this is exactly what did not happen. True, several people, including Richet (**34, 35, 36**), subsequently worked with Léonie, but in effect did nothing more than confirm what had already been done. Janet himself, after stopping at the brink of originality and perception, suddenly found the hysterical patients of the Saltpétrière much more interesting, and hied himself thither to the complete neglect of any further work with Léonie. Nowhere in his two papers does he tell us anything that a psychiatrist might contribute to the understanding of this most remarkable subject—nothing about her history, her personality, her peculiarities (if any), or any of the truly psychological corre-

[2] It is almost a certainty that Freud, who was studying with Charcot at this time in Paris, was at these meetings, although he never in his later papers on telepathy or anywhere else made reference to this work.

lates of her gift and performance. He hit the jackpot but walked away without picking up his winnings.

All he did was to stimulate a few further anecdotal papers on the same subject—hypnotism at a distance, so-called—by others in the same year (**1, 23, 24**). But then came a great calm, and nothing more was heard of this apparently fruitful area of investigation. (Richet himself did not pursue it, suddenly finding himself more interested in the calculus of chance.) In 1925, Janet, writing from a safe eminence (after, that is, he had circumspectly missed, besides the implications of his work with Léonie, the facts of the genetic, dynamic unconscious which he had in his grasp from his work with hysterics) confessed to a certain puzzlement (**30**): "Such a decadence, so rapid a disappearance after such high enthusiasm and such extensive developments is certainly surprising." One may well be surprised that Janet and others did not follow this preliminary work with all sorts of experimental variations and correlative studies, especially when such a rare and willing subject as Léonie lay to hand. But this illustrates as well as anything the tremendous, covert resistance of even psychiatrists to the subject. A hit-and-run attitude has characterized a good deal of the work done generally in parapsychology, and with very few exceptions psychiatrists who have touched so-called paranormal phenomena have made single contributions and have then retired from the field.[3]

Most observations by psychiatrists in the field of parapsychology have been concerned with telepathy and clairvoyance. Undoubtedly presumptive instances of these capacities must always have presented themselves in the ordinary course of psychiatric practice. But not until comparatively recently—not, really, until the work of Freud began to catch on around the beginning of the second decade of this century—was psychiatry liberated from its almost exclusive preoccupation with the frankly insane, where such occurrences might easily have passed unnoticed among the many odd and bizarre mani-

[3] Such resistance is characteristic not only of the "goats," who are reluctant to take the psi hypothesis seriously, but even of the "sheep," who appear to work most indefatigably to strengthen or broaden the hypothesis. Resistance, viewed psychoanalytically, may take many forms, of which the inhibition of scientific enterprise and ultimate withdrawal of interest is merely one. Excessive "scientificism" and methodological perfectionism are others which, although more easily rationalized, may be just as obstructive. It is safe to say that no worker in the field is free of unconscious resistance to psi that insidiously narrows his perspective and impedes his best efforts.

festations that constituted the psychiatrist's field of observation. With the growing acceptance of Freud's work, psychiatrists turned more and more to the study of people who were not actually deranged but who, in full possession of their mental faculties, suffered nevertheless from a variety of more or less disabling inner problems broadly termed "the neuroses." In this context—the lives and behavior of ordinary, average people who have a difficult struggle with personality imbalances and societal pressures—psi occurrences may be a little more readily recognized for what they are.[4]

At any rate, not until 1921, when Dr. Wilhelm Stekel of Vienna published his monograph on The Telepathic Dream (**45**), was any official psychiatric cognizance taken of the problem since the initial observations of Janet.[5] In this small monograph, Stekel cites a number of presumptively telepathic dreams observed in the course of his work with neurotic patients. His main observations have to do with the sensitizing effects of strong emotion, particularly love, jealousy and anxiety, in predisposing the agent and percipient to the telepathic event. Sleep, he felt, merely facilitates the reception of telepathic data, since in this condition the disturbances of the outer world are minimized. He believed that most people possess telepathic capacities which never show up in obtrusive ways, but which occasionally can be observed in dreams or in some of the manifestations occurring between people bound by strong emotional ties. "It is a well-known, often observed fact that women in love know when they are betrayed by their beloved partner." He states that he has observed women who experience a pain or some other symptom at the very moment they are being betrayed by their husbands. "Every individual emanates energy which charges the environment, impreg-

[4] It is still a moot question, as a matter of fact, whether the very ill and frankly deranged sufferers, the so-called psychotics, actually possess, latently or in manifestly observable form, the psi capacities that more normal people exhibit. The experimental findings on record all agree in showing evidence of about the same order of ESP capacity in psychotic patients as in the general normal population. (See the work of Margaret M. Price, Robert Shulman, and S. S. Van Wiemokly as reported in *Extrasensory Perception After Sixty Years* by J. B. Rhine *et al.*) The recent work of Urban and Köck may be cited to the same effect (**48**).

[5] In the first edition of his *Die Sprache des Traumes* (Munich: Bergman, 1911. Trans: The Interpretation of Dreams. New York: Liveright, 1943. See Vol. II, p. 551) Stekel cited several presumptively telepathic dreams he had observed in cases of siblings and married couples sleeping in the same room. Here, however, he brought no psychiatric point of view to his observations beyond noting that the telepathically transmitted material was always charged with affect (a broader and deeper term for the more subtle emotions).

nates it, so to speak. All of life's events are expressed in vibrations and rays which communicate themselves to the environment, 'charge' it. People emanate good and evil, love and discord."

Stekel deals in this monograph rather ambiguously with the possibility of prophetic dreams. One of the dreams he cites was his own, of the sudden death of his son three months before the actuality. Here, however, as in some of his other examples, the dream occurred in such highly symbolic terms that the actual element of prophecy would ordinarily be considered very dubious. And it is precisely at this point that Stekel, more the poet and gatherer of quick impressions than the penetrating, analytical thinker, missed (though just barely) the one insight that would have made his contribution, for all its fuzzy mysticism, outstandingly significant: he failed explicitly to establish the principle of symbolic identities, or to tie in with telepathic phenomenology the known facts of dream distortion. This remained for Freud himself to do.

Freud published his first paper on telepathy in 1922 (**22**), shortly after the appearance of Stekel's monograph, to which, as a matter of fact, he makes a casual reference in a slight footnote. It is not unlikely that Freud at this point could no longer refrain from bringing his bold, penetrating, incisive attack publicly to bear on a problem which he avowedly had been turning over in his mind for years. Not that he was at this time convinced of the reality of telepathy. He claimed that he himself had never had a telepathic dream and that he had never, during his twenty-seven years of work as an analyst, been in the position to observe a truly telepathic dream in any of his patients. Nevertheless, he had one observation at his disposal which, he felt, was provocative enough to justify the application of the psychoanalytic approach for whatever it was worth. This concerned a correspondent who had written that he had dreamed that his second wife had had twins and a day later found out that his daughter by his first wife, who lived some distance away, had that night given birth to twins, the confinement occurring about a month earlier than the expected date.

Now these facts—even assuming the correctness of them and excluding the usual avenues of knowledge and prediction—would in themselves be nothing to excite the interest of the ordinary psychical researcher. They would most likely lead only to the question

as to why the proud grandfather had thought them significant enough to report in the first place. The correspondence between the dream and reality was at best tenuous, since the dream represented the wife as giving birth to twins whereas in reality it was the daughter. Everything considered, there would be little here to provoke a second thought.

But Freud did give the matter a second thought. If, he reasoned, it could somehow be *assumed,* on the basis of the coincidence in time and subject matter, that the dream was telepathic, then the application of psychoanalytic principles could very well explain the troublesome substitution of the wife for the daughter.

One of the cornerstones of psychoanalytic theory was the well-known fact of distortion in dreams in order to conceal an unconscious desire or wish which, if represented in direct terms, the dreamer would find disturbing and unacceptable. This distortion, as well as whatever secondary elaboration of it was necessary to complete the disguise, is known as the *dream work.* With few exceptions, all dreams are brought to their final, *manifest* form by the alteration of their *latent* content through the dream work.

In the case in hand, one might well imagine the dreamer's unconscious wish to have been *not* that the wife give birth to twins, as represented, but that the daughter take the place of the wife, a wish that psychoanalysis has found to be very common on the part of middle-aged fathers. The dreamer, thus, might have had the latent wish to have been the father of his daughter's child. This would certainly have been unacceptable to consciousness. But assuming that the father had had telepathic percipience of his daughter's premature confinement, an event which would have threatened the not-too-stable repression of this latent unconscious desire, this wish would not have been represented as such but would have been denied and replaced in a dream by precisely the sort of thing which did emerge: "It is my wife after all whose offspring I wish to father."

It was thus feasible, asserted Freud, for psychoanalysis to unmask a possible telepathic event which otherwise would never have been recognized as such. If telepathy was a fact, he concluded (and he was still not unreservedly committed to the acceptance of this

proposition), then the laws of unconscious mental life could be taken for granted as applying to data telepathically perceived.

With this sweeping, brilliant generalization, Freud marked the path for a whole new development in the investigation of psi phenomena. Skeptical and reserved though he was at this time, and with only the flimsiest data to work with, he nevertheless made the one incisive stroke that Stekel had failed to make. Heretofore errors, misses and near misses—any departure from the strictly veridical—had been the bugbear of investigators in psychical research and had occasioned no end of trouble in both experimental work (for example, telepathic drawings, mediumistic utterances) and in the collection of spontaneous material (for example, apparitions, dreams, etc.). What Freud says, in effect, is: "Distortion of perception is one of the characteristics of mental functioning dominated by unconscious needs. But this distortion is purposeful and occurs along dynamic, deterministic lines. There is no reason to suppose that telepathic perceptions should be free from this universal effect. If one can, with the help of psychoanalysis, understand the reason for these distortions, one can uncover a much wider range of presumptively telepathic material than heretofore thought possible."

In this reviewer's opinion, there have been two major developments in the study of psi phenomena in recent years. One has been the careful refinement of statistical techniques for nailing down the evidence, as exemplified in the work of Dr. Rhine and his Duke co-workers. The other has been the application of a technique enabling psi phenomena to be viewed as part of man's total, needful, behavioral context. The credit for the impetus toward the second development, which is still but slowly getting under way, belongs clearly to Freud.

Freud returned to the subject of telepathy in 1925 with a somewhat more positive attitude (21). He centers his discussion around the analysis of a prophecy that had been given to one of his analytic patients by a fortune teller some years before. The patient had been told that by the time she was thirty-two years old she would have two children, but the prophecy had remained unfulfilled. At forty-three the woman was still childless. Freud found, however, that at the time the prophecy had been given, the patient's strongest

unconscious wish was to duplicate the feat of her mother who too had remained childless till almost thirty but who, long after hope had been abandoned, had had two children by the time she was thirty-two. The reading handed up by the fortune teller, suggested Freud, had less to do with actual future events than with the unconscious wish of the sitter which was picked up telepathically and handed back as the most satisfying prophecy.[6]

In this paper Freud also touched on certain conditions favoring the telepathic process. "I have often had an impression, in the course of experiments in my private circle, that strongly emotionally colored recollections can be successfully transferred without much difficulty. If one then proceeds to submit to an analytical examination the associations of the person to whom the thoughts have been transferred, correspondences often come to light which otherwise would have remained undiscovered. On the basis of much experience I am inclined to draw the conclusion that thought-transference of this kind comes about particularly easily at the moment at which an idea emerges from the unconscious, or, in theoretical terms, when it passes over from the 'primary process' to the 'secondary process.'" "All of this," concludes Freud, "has only this much to do with dreams: if there are such things as telepathic messages, the possibility cannot be dismissed of their reaching someone during sleep and being received by him in a dream. Indeed, on the analogy of other perceptual and intellectual material, the further possibility arises that telepathic messages received in the course of the day may only be dealt with during a dream of the following night. There would then be nothing contradictory in the material that had been telepathically communicated being modified and transformed in the dream like any other material. It would be satisfactory if with

[6] Priority for this explanation of an indeterminate number of errors on the part of professional sensitives must actually go to the psychiatrically oriented French physician, Dr. Eugène Osty who, in his *Supernormal Faculties in Man* (Dutton 1922. See Chap. V: "Psychological Determinism of Errors in Metagnomy") cited several cases of this sort and concluded that in most instances the sensitive merely voiced the repressed wishes of the client. "I commend to my fellow practitioners in psychiatry," he wrote, "the utilization of metagnomic subjects even in erroneous exercise of their faculty as more reliable and speedy methods of psychoanalysis than the tedious procedure recommended by Freud."

Osty, who was a prolific writer on psychical research, but who never made any particularly psychiatric contributions to the subject, here had more than the germ of an idea which would have gone far in introducing some order into the chaos of research with professional sensitives. It is not known whether Freud had seen Osty's contribution when he published his own data.

the help of psychoanalysis we could obtain further and better authenticated knowledge of telepathy."

Freud published nothing more on telepathy for eight years. Then, in 1933, he took up the subject again in his *New Introductory Lectures on Psychoanalysis* (20). Here he reviewed what he had already written on telepathy and cited a few more prophecies where analysis revealed that the fortune teller had merely given expression to the thoughts and particularly the secret, repressed wishes of the clients. The particular contribution at this time, however, was the demonstration of the use of the psychoanalytic method to disentangle from the threads of a patient's free associations during the analytic session evidence of the patient's unconscious, and presumably telepathically garnered, knowledge of a current event in Freud's life which was provoking in the patient considerable anxiety. Freud did not take the final step of attempting to liquidate the patient's anxiety by bringing to his consciousness the facts upon which this ostensibly irrational feeling was based, as he would have in the case of "transference" material normally elicited during the analysis. But he concluded by citing similar observations reported by Deutsch (4) and Burlingham (3) and stating that as far as he was concerned, further observations of this sort by other analysts, if confirmed, would "put an end to any remaining doubts of the reality of thought transference."

Of course, Freud was looking in the wrong place for his evidence. Data of this kind, however impressive it might be to analysts, would never measure up to the best standards of evidence demanded by the modern parapsychologist. But they do point in the right direction for the broader study of the complexities of telepathic functioning, once telepathy is assumed as a fact, in the relatively unhampered, unstructured, unfactitiously oriented *in vivo* situation.

Deutsch, in her paper, attempts to explain the mechanism of the emergence of the telepathic event in analysis in terms of then current psychoanalytic theory. Unfortunately, although she mentions the conditioning factor of the transference situation,[7] her theorizing

[7] The "transference" is the emotional dependency which develops during the analysis on the part of the patient toward the physician. An integral part of the transference situation is the living-out by the patient of his repressed unconscious strivings in relation to the analyst as if the latter actually represented the important figures in the patient's early life. The phenomenon of transference, discovered by Freud, is still largely unexplained.

was vitiated by an imperfect observation of what actually went on. She did not perceive the significance of what lay behind the seeming correspondence of her conscious preoccupations with those of the patients, since she attempted to explain events solely in terms of the unconsciously determined needs of the patient. This, as we shall see, is only half of the picture.[8]

In 1933 Hollós of Budapest published a paper embodying the most significant observations about the other half of the picture (**25**). Hollós found, from years of observation, that the telepathic event, as it manifested itself in the psychoanalytic situation, revealed just as much of the repressed unconscious material of the analyst as it did of the patient—as if the occurrence represented a merging or intersection of forces from *both* unconscious sources. In his examples he traces the distortions which make the end result of the telepathic process between the patient and the analyst a jointly elaborated event where the phenomenon represents a dynamic, unconscious *interplay* between the two and not simply an isolated act of perception on the part of one or the other. Similar observations, apparently independently arrived at, were published a couple of years later by Servadio[9] (**42**); and in 1942 Fodor[10] published a

[8] Other analysts have attempted psychoanalytic explanations of telepathic phenomena. Róheim (**37**) has emphasized the importance of the so-called primal scene "fixation" (secret, guilty observation of or fantasies about parental coitus) in its relationship to the *need* to function telepathically (seeing secretly, as it were) and Peerbolte (**32, 33**), in the same connection, has stressed the need to recapture the early mother-child relationship. While such explanations are necessarily limited in singling out one or another component from what must ultimately be a complexly overdetermined process, they at least attempt to relate psi phenomena to the deep, unconscious strivings of the personality. However, it is likely that the psi function can, from the proper observational standpoint, be related to any known psychobiological adaptive process, just as respiration is not simply the result of the action of the chest muscles *or* of the periodic discharge of the phrenic nerve *or* of the concentration of CO_2 in the medulla oblongata but is related to all these plus, ultimately, everything else that goes on in the body.

[9] Servadio, an eminent Italian psychoanalyst and also a trained observer and indefatigable worker in psychical research, has addressed himself to other problems shared jointly by the two fields. In a paper in 1932, part of which was given private circulation only (**41**), followed by a similar paper in 1933 (**40**), he made the first (and probably only) truly psychoanalytic observations on mediumistic productions and dissociative phenomena. He has also brought keen psychoanalytic insight to the psychological problems of telepathic hallucinations (**44**) and dowsing (**39**), and in 1947 he wrote on psychoanalysis and yoga (**43**).

[10] Fodor, a former Research Officer of the International Institute for Psychical Research in London, has also applied his psychoanalytic insight to the problem of the poltergeist (**17**) which he considers an *extrojected,* transient manifestation of a schizophrenic disorder that can be understood "only in the terms of the dynamic psychology which is based on the discoveries of Freud." The physical phenomena, even though actual, are nevertheless expressive of deep-lying psychological conflicts.

number of dreams analyzed in considerable detail to illustrate "that the clue to a complete understanding of a dream sometimes lies in an event which we cannot know about through the patient's associations alone and that, in some instances, we may find the missing clue by analyzing our own dreams in relationship to our patient" (**18**). (See also **19**.)

Quite apart from the value of these contributions of Hollós, Servadio and Fodor to parapsychology, they assuredly form the basis of a whole new approach to the psychoanalytic understanding of the patient's productions.

The psychiatrist who has concerned himself most with the theoretical aspects of telepathy is Ehrenwald. In a series of papers (**6, 8, 9, 11**) culminating in a book on the subject in 1948 (**10**) he puts forward views which may be summarized as follows. A necessary condition for telepathic functioning in the agent and/or the percipient is a state of psychobiological inadequacy or deficiency such as loss or clouding of consciousness (sleep, hypnosis, trance, fever, etc.), brain defect (for example, feeblemindedness), endocrine imbalance, physiological exhaustion, or the like. This Ehrenwald terms the theory of "minus function." To account for the errors and distortions in telepathic functioning he posits a "scatter" theory in which some inherent lack of precision is attributed to the telepathic process. Addressing himself to the question of the schizophrenic psychoses, he suggests that paranoid psychotics may be unable to differentiate in consciousness the material arising from their own psychological metabolism (autopsychic sources) from ideas, emotions, etc., coming from outside (heteropsychic sources), and that paranoid delusions may actually have a basis in telepathically perceived fact.[11] If this is so, Ehrenwald argues, it is possible that catatonic stuporous conditions might represent a refractory phase, a complete shutting-out process on the part of the organism whose barriers against the invasion of anxiety-provoking heteropsychic material would otherwise be too weak and ineffectual for normal mental functioning.[12]

Ehrenwald's speculations have met with some criticism from

[11] This possibility had also been suggested by Fodor (**18**).

[12] Bendit (**2**) has discussed the possibility in the neuroses of unbidden invasions of anxiety from outside sources. He claims that making the patients aware of this possibility is in itself a considerable therapeutic aid.

his fellow-workers in the field (**14, 46, 47**). It has been pointed out that the "minus theory" is a somewhat too narrow conception which does not take many contrary facts into account, and that the "scatter theory" ignores the dynamic determinants of the errors and distortions that occur. As for his speculations on the role of psi in the schizophrenic psychoses, it has been generally held that the meagre evidence at hand does not warrant such conclusions.

Several papers by Eisenbud have been concerned mostly with the further delineation of the phenomenology of telepathy as it manifests itself in the psychoanalytic situation, as well as with the possible application of such knowledge to the technique of psychoanalysis. In a paper published in 1946 (**15**) he attempts to demonstrate the far-reachingness of telepathic interplay in analysis, not only between patient and analyst but between the several patients as well.[13] He also illustrates in considerable detail the involvement of the analyst's own repressed unconscious material in such interplay[14] not only in dreams but in "free" associations and "acting out" as well, and even in the telepathically conditioned interplay between patients where the analyst does not, on superficial inspection, appear to be involved at all. Concluding that the psi process is a thoroughgoing part of the total behavior of the individual, and as much of a determinant in the actions and thoughts of the patient as other types of stimuli, Eisenbud attempts to demonstrate the technical value in psychoanalytic procedure of making the patients aware of what is going on at this level. He suggests that the proper handling not only of transference reactions but also of the *countertransference* (the unconscious attitudes of the analyst towards his patient) can best be facilitated by the fullest use of presumptively telepathic material for both self-insight on the part of the analyst and interpretation to the patient.[15]

[13] This last was best brought out in the publication the following year of a pair of interlocking dreams of two patients where the exquisitely precise nature of the telepathic process, once the distortions were psychoanalytically unmasked, was revealed (**13**).

[14] The thesis that the telepathic episode in analysis is a function not only of the repressed unconscious material of the patient but of the analyst as well is amply confirmed by Pederson-Krag (**31**) who also attempts to deal with the question of why psi has developed as such an inhibited function in the human species.

[15] The analytic contributions of Eisenbud, Fodor and Pederson-Krag, as well as the telepathic hypothesis itself, were severely attacked by Ellis, a clinical psychologist (**16**). The defendants were given an opportunity to reply in the same issue. (See also (**12**).)

Current psychiatric work with parapsychological phenomena seems to be dividing itself into two main approaches, the microscopic and the macroscopic. It is certain that the "microscopic" psychoanalytic investigation of the modes of manifestation and the conditioning role of psi in the everyday affairs of life will continue to be a fruitful field of inquiry for an indefinite period ahead. On the macroscopic front, personality studies on sensitives, such as suggested by Ehrenwald (**5, 10**), and score correlation studies by means of the projective technique method of personality appraisal employed by Schmeidler (**38**) and Humphrey (**26**) will probably be vigorously pushed. Investigations such as those being carried out by Urban of Austria are also promising. In a preliminary report of this work (**48**) it would appear that there is a significant correlation between various forms of psychiatric treatment and scoring success on ESP card tests. Such attempts at correlation of mental and physical disorders with aspects of psi phenomena, also begun by Hyslop (**27**) and Ehrenwald (**7**), are indicative of the current trend toward viewing psi phenomena as not above and beyond the corporeal side of man but as inextricably linked with all aspects of his psychobiological functioning.

Continued work in parapsychology from the psychiatric point of view and continued work in psychiatry from the parapsychological point of view can only prove mutually helpful. It is conceivable, moreover, that such cross-fertilization might even prove mutually revolutionary. Nothing would be lost in such a shuffle beyond a certain amount of methodological rigidity on both sides.

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A COMPARATIVE STUDY OF PERFORMANCE IN THREE PSI TASKS

By Robert H. Thouless

ABSTRACT: In this experiment one clairvoyance test and two different types of precognition tests were carried out concurrently. The author tested himself by calling a total of 432 times "down through" a deck of ESP cards. The clairvoyance test produced a significantly negative total score; one of the precognition tests produced a chance score; and the other, a significantly positive score. The author attributes these differences in scoring rate to the differences in his familiarity with the tests and to the inhibitions which may have developed as a result of this familiarity. Hence, chronological decline may be specifically related, at least in part, to a particular test instead of to scoring in general.

Although the total results on the three methods differed greatly, the run scores on all three tended to vary together from day to day to a significant extent. That is, an increase (or decrease) in hits at some time during one test was accompanied by an increase (or decrease) in hits in the other two. These correlations, Dr. Thouless suggests, indicate that positive, negative, and chance scoring may be the result of positive and inhibitory forces in varying states of balance.—Ed.

Procedure

In the investigation here reported I have made use of three procedures two of which were used in an experiment by Mr. Skibinsky in which I acted as subject in 1948 at the Duke University Parapsychology Laboratory. In Skibinsky's experiment, the expected order of a pack of ESP cards was written down by the subject and his prediction was scored against the order of the pack after it had been cut at a randomly determined point. This experiment (or any variant of it which includes the randomly determined cut after the guess) is particularly well suited for self-experimentation since the possibility of any knowledge of the final order of the pack by normal sensory or inferential means is completely eliminated by the cut. Even if the subject had known the exact order of the pack before the cut, his mean chance expectation of right answers after the cut would be only five, just as it would be if he had no such knowledge.

All the experiments which follow were done by myself acting as both subject and experimenter with no other person present. A closed deck of ESP cards was used (that is, a deck with five of

each of the five symbols). For estimates of significance, I used the standard deviation appropriate to the closed deck (2.04 instead of 2.00 per run of 25).

My method of guessing was to write down the symbols as I supposed they would occur in the pack when the check was made. As I wrote the symbols down, I repeated their names subvocally. I had no visual images of the card faces and I made no special effort to fulfill the particular task of prediction or clairvoyance of the run I was engaged in. I knew what that task was but I made no special effort to keep it in mind while I was writing my guesses.

The random cut was made in the following way: I threw three dice marked, respectively, *A, B,* and *C*. The fall of *A* determined which of six pages of two-figure random numbers I would use; the fall of *B* determined the column on that page from which the random number would be taken; and the fall of *C* determined the row. The number so selected was divided by 25 and the remainder was the number of cards to be removed from the top of the pack in the cut. Thus the dice might fall 2, 3, 5, which would mean that the number selected was that at the intersection of the third column and the fifth row of the second page of my tables. Supposing this number to be 46, division by 25 would leave a remainder of 21. The pack in this case must therefore be cut by removing the top 21 cards and placing them under the bottom four.

Three types of experiments were performed:

A. One was the ordinary DT technique. In these tests the pack was thoroughly shuffled and a random cut was made in the manner described above. The pack was laid face downward on the table and left undisturbed until the check. The calls were then made in the manner already described and these were checked with the order of the pack without further cutting.

B. In the second method, the pack was shuffled and placed face downward on the table. The calls were written down, the random cut was made, and the calls were then checked by comparison with the order of the pack after this cut.

C. In the third type of test the pack was shuffled and placed face downward on the table. The calls were then written down, the pack was again thoroughly shuffled, and the random cut was

made as before. The check was made by comparing the calls with the order of the pack after this shuffle and cut.[1]

The difference between the experiments, therefore, was that *A* was done under conditions which could be explained by contemporary clairvoyance without precognition. Success in *B* necessarily requires an explanation by precognition. Success in *C* would require precognition of the effect of a more complex system of events than that of *B*—the effect of shuffling as well as cutting.

It may be asked how I could be sure that the shuffling was adequate to produce a new order without systematic relation to the preceding one. It was as thorough as I could make it, and no systematic relation between the shuffled order of the cards and their previous order was detectable. It was not necessary for the objectives of the experiment, however, that shuffling should be thorough. Adequate precautions were fully ensured, as has already been pointed out, by the random cut. If, for example, the pack were not thoroughly shuffled in experiment *C*, this would merely mean that its conditions would approximate those of experiment *B*. Since the scoring was very different in experiments *B* and *C*, it is clear that the shuffling was sufficiently good to make them effectively different experiments.

On each of 144 experimental occasions three runs were made, one by each method. In order that a comparison might be made between the scoring rates of the three methods, the serial order of the methods was systematically varied, the orders *ABC, CAB, BCA* being used in succession. The total length of the experiment was 144 runs by each method, this total length having been finally decided at the one hundred and eighth experimental occasion.

Results

Altogether, 432 runs of 25 cards each were completed. The results of the experiment are shown in the accompanying diagram in which are plotted the cumulative total deviations from mean chance expectation at the end of each set of six experimental occasions (that is, of six runs by each method).

[1] Although the random cut was used in the B series from the beginning, it was not introduced into the A and C series until the twenty-second set of runs. All conclusions with respect to A and C would, however, hold if the first 21 sets were omitted.

The general direction of results may be easily seen from Figure 1. In *A* the scoring was fairly consistently below mean chance

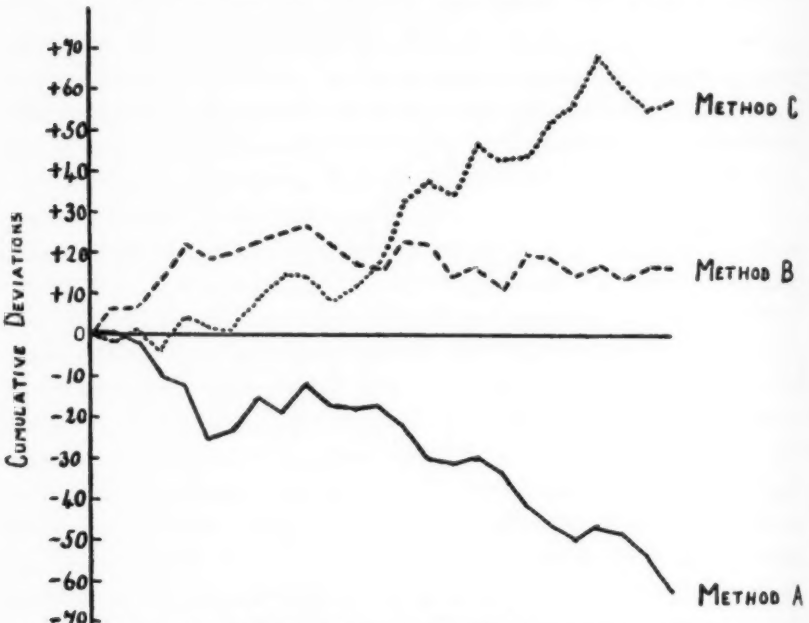

FIG. 1. Graph showing the cumulative deviations for all three methods at the end of each set of six experimental occasions (that is, at the end of six runs for each method).

expectation, reaching at the end the significant negative deviation of −62. In *B* the scoring began by being positive and reached a value of +23 at the twenty-third run. After that, the rate of scoring remained close to the chance level until the end of the experiment when the deviation was +17. In the *C* series, on the other hand, the scoring at the beginning was only at the chance level. Positive scoring started, however, after the thirty-sixth run and continued thereafter at a low rate until about halfway through the experiment when the rate of scoring increased until the cumulative deviation reached the final total of +57, which, like *A*, is satisfactorily significant.

Table 1

TOTAL RESULTS FOR EACH OF THE THREE METHODS

Method	Runs	Total Dev.	CR	P
A.....	144	−62	2.54	.01
B.....	144	+17	.70	.5
C.....	144	+57	2.33	.02

Table 1 shows the final totals for the three methods of experiment with critical ratios and P values (that is, the likelihood that a deviation of the observed value or more would occur by chance in either direction).

The negative deviation for *A* and the positive one for *C* are clearly significant. It is not so clear that the initial deviation by the *B* method could not have been a chance effect. The final deviation for *B* is (as might be expected) altogether insignificant. It remains possible that the initial tendency to positive scoring in *B* was a real one, afterwards swamped by the much longer sequence with chance scoring. We have, however, no altogether satisfactory way of assessing the significance of such an initial scoring tendency followed by a long series of chance scores. At the end of the twenty-third session the deviation for *B* was +23 which, if it could properly be considered alone, would give a critical ratio of 2.35 with P = .02. To accept this as a measure of the significance of the initial deviation would, of course, be erroneous since it would involve the fallacy of selecting the point of testing most favorable to the hypothesis under consideration. It would seem that the best way of assessing the significance would be by finding an answer to the question: "What is the likelihood that in a chance series of 144 sets of observations the P-value of .02 (or less) will be obtained at least once?" I am not able myself to find a way of answering this question, and statisticians to whom I have referred it have also not been able to give me an answer. Obviously the required likelihood must be greater than .02 but it may not be much greater. Provisionally we may consider the initial positive score in *B* as probably a real effect, although with much less confidence than we can treat the *A* and *C* deviations as real effects.

The difference of 119 between the final *A* scores and *B* scores is

highly significant ($P < .001$). A complete record of score differences and of their significance is given in Table 2. The critical ratios in this table were calculated from the observed standard deviations; use of the ordinary formula based on the "theoretical" variance would be erroneous, since the series were not uncorrelated (as will be shown in a later section) and are not centered around a mean score of 5.00.

Table 2

Comparison of Scoring Level among Three Methods

Methods	Difference	SD	CR	P
B - A.....	+ 79	30.65	2.57	.01
C - A.....	+119	33.0	3.60	.0003
C - B.....	+ 40	30.1	1.33	.2

Discussion

The main interest of these results is that we have three experiments going on side by side showing different stages of the well-known process by which positive success is replaced by inhibition which culminates in negative scoring. One experiment shows fairly consistent negative scoring; one shows an initial tendency to score positively followed by chance scoring; and the third, initial chance scoring followed by fairly consistent positive scoring.

The first conclusion to be drawn from these results is one of considerable practical importance. It is generally assumed in discussions of chronological decline in scoring that this is a general effect (such as boredom or some other factor affecting the experiment as a whole). While these results do not rule out the possibility that there is also a general effect causing progressive extinction of positive scoring, they show clearly that the effect may be specific to the particular form of test employed, since one method may produce negative scoring while another is producing positive scoring. I have also observed this specificity of inhibition to a particular form of test in PK tests (not yet published). The practical conclusion is that if we want to maintain high scoring in an experiment, the best way is to use a variety of experimental methods.

An odd feature of the results is that positive scoring is obviously

not related to the apparent difficulty of the task. It is the apparently simplest task (*A*) that scores negatively, while the most complicated task (*C*) shows the highest positive score. My original idea in including *C* was that this was a task in which it was impossible to succeed, so that I should get merely chance results for comparison with the positive results I hoped for from *A* and *B*.[2] It would, of course, be rash to conclude that the complex and apparently difficult task is necessarily one in which the subject is more likely to succeed; it may well be that the determining factor was that *C* was an entirely new task for me while the others were not.

The differences in success with the three tasks cannot have been due to my expectations since they did not follow these. When I started I expected to succeed in *A;* I thought I might succeed in *B* at a higher rate than in *A;* while I was convinced that I would fail in *C*. Initially the first expectation was falsified while the other two were fulfilled. By the time I had done the first twenty runs or so, I expected to go on scoring negatively in *A,* to go on scoring positively in *B,* and to go on getting chance scores in *C*. The first of these expectations was fulfilled and the other two were not.

I would suggest tentatively that this was the result of the different degree of chronological decline for the different tests which resulted from the varying amounts of previous experience of doing them. I have done tests of the *A* form for many years, since the DT technique was first described by Rhine. I have therefore had time to develop a complete inhibition for this form of test. I have done *B* a good many times since I first met the test a year ago, so in *B* it may be that an inhibition started developing in the course of this series of experiments (on the assumption that the initial positive deviation in *B* is a real effect). The *C* form of the experiment was, however, quite new to me; I had never done it until this series of tests started, so it had had no chance of being inhibited. It is curious that success was not immediate but that there was a flat part of the curve before scoring started. There is insufficient evidence to assert that this preliminary failure is real but I have noticed indications of a similar warming-up period in new forms of PK tests, and it is perhaps a thing to look out for in the future. If I

[2] This opinion was due to my ignorance, at the time, of the fact that one of the early successful experiments on precognition at the Parapsychology Laboratory at Duke was done under these conditions (**2**).

had continued the experiments long enough, I might have induced negative scoring in B and chance scoring in *C*. The 144 repetitions of the test were, however, all that I felt I had time for at present.

The results of this experiment also seem to bear on the suggestion that has been made that success in a clairvoyance task may be due to precognition of the check. If precognition of the check were the cause of success in these tasks, one would expect that all would succeed to the same extent since they would then be equivalent tasks. Negative scoring in A implies that the psi process involved in that task must be of a different nature from the one involved in *B* and *C*, in which the scoring is either in accordance with mean chance expectation or positive.

A further point of interest in the results is the indication of a positive correlation between the different tasks. This can be seen in the diagram as a tendency towards parallelism between dips and rises in the three curves. In order to determine whether this appearance of positive correlation was real, a coefficient of correlation was worked out by the Bravais-Pearson formula between each method for the 144 experimental occasions. The results are shown below.

Methods Compared	Correlation coefficient	P
A — B	r = +.11	~ .2
B — C	r = +.17	~ .04
A — C	r = +.062	~ .5

Although there is a suggestion of positive correlation of each method with each of the other methods, in no case does the coefficient reach the value necessary for statistical significance (r = .19). No conclusion can therefore be drawn from this table as to whether the tendency to correlation is a real one. A more sensitive test may be applied, however, by a method which takes into account all three methods together. This can be done by applying the test for significance for the intraclass correlation between the three methods by the method of analysis of variance. The results are shown in Table 3.

The large ratio of 7.42 of methods variance to error variance merely shows that the difference between the scores by different methods is significant; it repeats in another form the information contained in Table 2. The figure relevant to the present question is the ratio 1.35 of variance between occasions to error variance.

Table 3

INTRACLASS CORRELATION AMONG THE THREE METHODS AS SHOWN BY AN ANALYSIS OF VARIANCE

Σ Squares	Degrees of freedom	Variance	Ratio of variance to error variance	P
Total 1693.67....	431			
Occasions 662.33....	143	4.63	1.35	.02
Methods 50.93....	2	25.46	7.42	<.001
Error 980.41....	286	3.43		

This is significantly greater than 1 ($P = .02$), showing that there is some factor tending to make scores of the three tasks at any one time more like each other than they would be by chance alone. In other words, there is a significant tendency to positive correlation among the three tasks.

The fact of covariance between the tasks is not, of course, surprising; correlation between psi tasks has been reported before (1). I did, however, find its direction surprising. I had expected a negative correlation between a task scoring positively and one scoring negatively. This follows from the consideration that the subject needs psi either to score significantly above chance or to score significantly below chance. If on some days my psi capacity was working well I should have expected, therefore, to score high on *C* and to have a large negative deviation on *A*, while on a day when psi was working badly I should have expected to have small deviations in both tests. The correlation between a task scoring positively and one scoring negatively would then have been negative; a large number of hits in the one would be accompanied by a small number in the other.

This was not the case; when a small number were right in *A* the tendency was for the number right in *C* also to be lower than usual, and when a large number were right in *C*, the number right in *A* also tended to be larger than usual. The reasoning on which the opposite expectation was based must, therefore, be basically wrong. There is another way of looking at the matter which would lead to the expectation of a positive correlation between a task scoring positively and one scoring negatively. Let us suppose that

any score is the resultant of two sets of forces, one leading to positive scoring and the other to negative, and that the difference between a task scoring positively and one scoring negatively is simply that the first force predominates in the one and the second in the other. Then the day-to-day variation in scoring may be due to the fact that one force is stronger on one day and the other on another. When the force producing positive scoring was stronger it would produce a larger number right, both in the task scoring positively and in that scoring negatively; while an increase in the inhibiting force would produce a larger negative deviation in the task scoring negatively and a smaller positive deviation in the task scoring positively. The correlation between the two tasks would thus be positive, as is actually observed.

Furthermore, the ordinary way of looking at the matter would lead one to expect that there would be no correlation at all between a task scoring about the chance level and one scoring either positively or negatively. For the task in which there are no significant deviations is generally regarded as one in which the number right is determined by chance alone and not at all by psi, so its score cannot be affected by day-to-day variations in psi efficiency. But in the present experiments, there does appear to be positive correlation between a task that is succeeding and one that is showing apparently chance scores. For example, in the first 36 occasions before positive scoring had started in *C*, the correlation between the *C* scores and the *A* scores was +.34, which is separately significant ($P < .05$). This strongly suggests that a task in which there is no significant deviation from mean chance expectation may not be one in which the scores are unaffected by psi but simply one in which the forces making for positive scoring and the opposed inhibitory forces are nearly exactly balanced.

It is obvious that the evidence on which these speculations are based is not of great strength, and they are put forward with no strong conviction of their truth, but rather as a tentative hypothesis to be confirmed or refuted by the research of others. If, however, it can be confirmed that a task scoring positively correlates positively with a task scoring negatively, and that a task showing no tendency in either direction is positively correlated with a task scoring either

positively or negatively, then the idea of all scores being the resultant of positive and inhibitory forces will be strongly indicated.

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ESP SUBJECTS RATED BY TWO MEASURES OF PERSONALITY

By Betty M. Humphrey

ABSTRACT: In the previous issue of this Journal, Dr. Humphrey reported that when subjects were divided into "midrange" and "extreme" groups on the basis of their answers to the Stuart Interest Inventory, there was a significant difference between the ESP scores of the two groups. Now she has combined the interest measure with another personality rating, that of expansion-compression based on qualities in freehand drawings made by the subjects. In an analysis of sixteen clairvoyance card series, she found that subjects who received both expansive and midrange ratings gave positive ESP results. On the other hand, the subjects who received both compressive and extreme ratings scored significantly below the expected chance average in the ESP tests. In spite of the fact that there was a smaller number of cases to be contrasted when both measures were combined, the difference between the scores of these two groups was still highly significant, with odds against chance of approximately 2,000 to 1. Moreover, the difference between the average scores of the two groups resulting from the combination of two personality measures was greater than that obtained when each measure was used alone. Additional analyses show that the difference was not due to a few outstanding subjects in each group, but that a relatively large number of subjects in each contributed to the effect.—Ed.

THE PERSONALITY attributes of subjects in ESP tests have been the center of interest in many recent researches in parapsychology. Several personality-measuring devices have been found effective in separating high-scoring ESP subjects from low-scoring ones. The purpose of this report is to present the results of a study in which two measures of personality were used in combination. Taken singly, each of the two had, with varying degrees of success, separated high- and low-scorers in ESP tests. It was hoped that selection of those subjects who were placed in the high-scoring or low-scoring group by *both* measures would result in an even more effective separation than had been achieved when each measure was used alone.

One of the measures of personality used in this study was based on the Stuart Interest Inventory, which was reported in the September issue of this JOURNAL (2). Briefly described, the interest inventory itself consists of sixty items of common interest which subjects in ESP experiments are asked to check by indicating on

a five-point scale the degree of their interest in each item. Dr. C. E. Stuart had devised a method of scoring the inventory which divided the subjects in a given series into two groups: (1) the midrange group, and (2) the extreme group. As readers of the previous report will recall, when Stuart's scoring method was applied to the results of sixteen series of card-calling tests of clairvoyance, the subjects in the midrange group gave a mean ESP score which was significantly higher than that obtained by the extreme group (CR of the diff. = 3.19).

The second measure of personality used in the present report in combination with the Stuart Interest Inventory is the expansion-compression score based on the form quality of freehand drawings. This measure has also been described in detail in other reports (**3, 4**). The expansion-compression measure is based on *how* a subject draws and not *what* he draws. Simply stated, an expansive person is one who draws freely and uninhibitedly, and with some imagination. The compressive person, on the other hand, is one whose drawings show that he has been tense, inhibited, and unable to express himself freely. The expansion-compression criterion has proved especially useful in separating high- and low-scoring subjects in ESP tests based on drawings instead of card-calling responses (**1, 3, 4, 9**). However, the measure has also been used with varying degrees of success in the attempt to separate high- and low-scoring subjects in ESP card tests, the expansion-compression judgments being based on drawings made by the subjects in the same session as the card tests (**5, 7**). The procedure most often used was to have subjects make one or two drawings in the attempt to identify concealed target pictures. Then the subjects were given several runs of ESP card tests. The drawings were collected solely for the purpose of the expansion-compression rating, and were not evaluated for evidence of ESP. The ESP card run scores of subjects who obtained expansive ratings were then compared with the scores of those whose drawings were classified as compressive.

In the sixteen series of clairvoyance card tests to which the Stuart Interest Inventory measure was applied, expansion-compression ratings of the subjects were also secured. In fact, the main purpose of most of the series was to explore further the usefulness of the expansion-compression criterion, and to study the effect of

variations in conditions and instructions on the subjects' drawings. In the later series where more marked variations in procedure were introduced, the expansion-compression criterion was not generally effective in separating high-scorers in the ESP tests from low-scorers. Nevertheless, it was thought worth while to combine the expansion-compression measure with the Stuart interest measure for all available series in order to see if the combination produced a more effective separation of the subjects into high- and low-scoring groups.

If the two measures of personality are highly correlated, we would hardly expect a combination of them to be more effective in separating high- and low-scorers than either measure alone; both personality ratings would be measuring essentially the same characteristic and so they would give virtually the same results in combination or alone. On the other hand, if the two measures of personality are uncorrelated or only slightly correlated, it is probable that a combination of the two would result in a larger difference between high- and low-scoring groups than would either measure alone. Statistical analysis of the present data suggests that the two measures dealt with here are uncorrelated.[1]

On the basis of the previous results obtained with these two measures, then, we may expect that subjects who are expansive in their drawings and who also fall in the midrange category in terms of the interest inventory will give the highest average ESP score, and that subjects who are compressive in their drawings and who also fall in the extreme category will give the lowest average ESP score. Subjects who are expansive and in the extreme group, as well as subjects who are compressive and in the midrange group, will be expected to give ESP scores which are between the high and low extremes mentioned above. It is by no means inevitable that the combination of measures will give results in accordance with these expectations; it is possible for the ESP scores to be so distributed that the expectations will not be borne out. What we are seeking is a more effective research tool for predicting which subjects will score high on an ESP test and which will score low.

[1] A chi-square test of the independence of the two measures gives a chi square of .77, which with one degree of freedom gives a probability of .38. On the basis of this result there is no reason to suspect any degree of association between the two measures.

Whereas it may appear logical to conclude that a combination of two measures will be a better tool than either one alone, we cannot afford to rely solely on logic but must actually apply the combination before we can be sure that it is more effective.

The Data and the Methods of Analysis

The report on Stuart's work with the inventory measure (2) described the essential conditions of the sixteen series with which we are concerned in the present report. The series are divided into two sections: (1) The first eight series are known as the First Section and constitute the records to which Dr. Stuart himself had applied the interest measure. (2) The Second Section is comprised of eight additional series to which I applied the interest measure.

The procedure used in the various series and the precautionary measures employed were described in the report on the interest inventory (2) and in previous reports of some of these series (7, 9); therefore these details will not be repeated in full here. In fifteen of the series the subjects were tested in groups. In only one series were they tested individually. These series are listed in Table 1, together with certain basic information as to the experimenter, number of subjects, number of runs per subject, and so forth. (The series in which Dr. Burke M. Smith acted as experimenter will hereinafter be referred to with the letter *S* preceding the series number. The letter *M* indicates that Miss Elizabeth McMahan was the experimenter, while *R* refers to Dr. J. B. Rhine, and *H* to myself.) The table also lists the specific type of clairvoyance test used in each series. *BT* refers to a procedure in which the subjects recorded their respective calls for each card as it was held apart from the deck by the experimenter; *DT* designates the test in which the subjects were asked to call "down through" a pack of cards which was not touched until the run was completed. The procedure referred to in the table as *5 x 5* was a special technique in which the 25 ESP cards, each enclosed in an opaque envelope, were arranged in five rows, five cards to a row. The board to which the cards were attached in this arrangement were set before the subjects, who recorded their calls on special record sheets containing five rows and five columns. In all series the cards were screened from the subjects' view, and the order of the cards was not known to the experimenter.

Table 1

GENERAL SUMMARY OF INFORMATION CONCERNING SERIES

FIRST SECTION

Series	Experimenter	Number Subjects	Number "Sessions"	Number Drawings per "Session"	Number Runs per Subject and Procedure	Total Runs	Dev.
S1.....	Smith	36	2	1	4 Runs BT	144	− 4
S2.....	Smith	33	2	1	4 Runs BT	130*	−17
S3.....	Smith	25	2	1	4 Runs BT	98*	+13
S4 Ma.....	Smith	34	1	4	2 Runs BT	68	+12
S4 Sm.....	Smith	29	1	4	2 Runs BT	58	− 3
S5.....	Smith	37	1	2	2 Runs BT	74	+ 7
S6.....	Smith	32	2	1	4 Runs BT	128	−55
M1.....	McMahan	32	1	4	4 Runs BT	128	− 2
Total...		258				828	−49
						CR = .85	

SECOND SECTION

Series	Experimenter	Number Subjects	Number "Sessions"	Number Drawings per "Session"	Number Runs per Subject and Procedure	Total Runs	Dev.
S7.....	Smith	40	1	1	2 Runs DT	80	+32
S8.....	Smith	29	1	1	2 Runs BT	58	− 6
R6.....	Rhine	37	1	1	2 Runs BT	74	+ 4
R7.....	Rhine	41	1	1	1 Run 5 x 5	41	− 7
R8-A.....	Rhine	36	2	1	4 Runs DT	142*	+ 1
R8-B.....	Rhine	40	2	1	4 Runs DT	160	+11
H2.....	Humphrey	28	1	1	2 Runs BT	56	−10
H4.....	Humphrey	33	1	1	2 Runs DT	66	+16
Total...		284				677	+41
						CR = .79	

*One subject made two runs instead of the usual four runs.

The method of scoring the interest inventory and of determining midrange and extreme groups will not be repeated here since it was given in detail elsewhere (**2, 8**).

Drawings which were to provide the basis of the expansion-compression ratings were obtained from the subjects in all sixteen series. In some series the subjects made their drawings before taking the card tests, while in others the card tests were given first. The number of drawings made by each subject varied from series

to series. Table 1 shows the number of drawings made by each subject and the number of "sessions" for each subject. The term "session" is used here to refer to a specific number of tests carried out under the same conditions and not in the usual sense of a separate experimental occasion. In some series (S1, S2, S3, S6, R8-A, R8-B) the first "session" was followed by a second, done under different conditions and in some cases carried out immediately after the first. The term "session," therefore, is used to refer to sections of the data which were to be considered separately in the expansion-compression analysis. The expansion-compression ratings were made independently for the different sessions since the change in conditions was usually introduced in order to note its effect on the expansion-compression rating.

The ratings for expansion-compression were always made by judges who did not know the ESP scores of the subjects. Both Dr. Stuart and I independently classified the drawings as expansive or compressive for all series except two (R8-A and R8-B). The ratings were made according to the procedure described previously in other reports (**3, 4**). The final rating assigned to each subject in fourteen of the series was determined by averaging the ratings given by the two judges. In Series R8-A and R8-B, where only one judge was used, the subjects' ratings were those assigned by me alone.

In combining the expansion-compression rating with the inventory measure, the subjects in all series were separated into four groups: (1) subjects who were expansive according to the rating of their drawings and in the midrange group according to the interest inventory, (2) subjects who were expansive and in the extreme group, (3) subjects who were compressive and in the midrange group, and (4) subjects who were compressive and in the extreme group. Our main interest centers in the first group, which, on the basis of both personality measures, would be expected to have a high average ESP score, and in the fourth group, which would be expected to give a low average ESP score.

According to routine practice, all data and computations have been rechecked at least twice.

Results

Review

Before presenting the results of the combination of the personality measures we should first review the results obtained when each measure was used alone. When the interest inventory measure was applied to the total of 1,427 runs available (2), a significant difference was found between the ESP results of the midrange group of subjects and those of the extreme group (CR of the diff. = 3.19). As Table 2 shows, the midrange group gave a positive deviation, while the extreme group gave a negative one. The significance is centered mainly in the First Section of the data (CR of the diff. = 3.18). Although the trend of the Second Section is similar, this section contributes little to the significance of the whole.

Table 2

ESP Results for Midrange and Extreme Groups

Section	Midrange Group				Extreme Group				CR_d
	Number Subjects	Runs	Dev.	CR	Number Subjects	Runs	Dev.	CR	
First.....	120	378	+ 56	1.44	119	378	−120	3.09	3.18
Second.....	138	333	+ 52	1.42	140	338	− 13	.35	1.26
Total.....	258	711	+108	2.03	259	716	−133	2.49	3.19

Results of the separation of ESP card runs on the basis of the expansion-compression ratings have previously been presented for only six of the series, Series S1, S2, S3, S4 Ma, S4 Sm, and M1 (7, 9). Therefore the card run scores of the expansive subjects and those of compressives for all sixteen series are presented in Table 3. The expansive subjects as a group gave a positive deviation, while the compressives obtained a negative deviation. The results, however, are not statistically significant for all sixteen series pooled (CR of the diff. = 1.65). As was the case with the interest inventory measure, the largest effect occurs in the First Section, where the CR of the difference between the mean ESP scores obtained by expansive and compressive subjects is greater than that for the total. In the Second Section there is relatively little dif-

Table 3

ESP Results for Expansive and Compressive Groups

First Section

Series	Expansive			Compressive		
	Number of Subjects*	Runs	Dev.	Number of Subjects*	Runs	Dev.
S1	22(11)	66	+13	25(11)	78	−17
S2	24(11)	74	− 2	20(12)	56	−15
S3	14(9)	38	+22	20(10)	60	− 9
S4 Ma	13	26	+ 6	21	42	+ 6
S4 Sm	10	20	− 9	19	38	+ 6
S5	8	16	+10	28	56	− 3
S6	14(12)	32	−15	30(12)	96	−40
M1	13	52	+16	19	76	−18
Total	118(43)	324	+41	182(45)	502	−90
		CR = 1.14			CR = 2.01	
			$CR_d = 2.15$			

Second Section

Series	Expansive			Compressive		
	Number of Subjects*	Runs	Dev.	Number of Subjects*	Runs	Dev.
S7	12	24	+10	28	56	+22
S8	9	18	− 2	20	40	− 4
R6	11	22	+ 9	26	52	− 5
R7	21	21	+ 2	20	20	− 9
R8-A	18(7)	58	− 6	25(8)	84	+ 7
R8-B	18(11)	50	+16	33(12)	108	− 8
H2	14	28	− 9	14	28	− 1
H4	15	30	− 3	18	36	+19
Total	118(18)	251	+17	184(20)	424	+21
		CR = .54			CR = .51	
			$CR_d = .11$			

Both Sections

Series	Expansive			Compressive		
	Number of Subjects*	Runs	Dev.	Number of Subjects*	Runs	Dev.
Total	236(61)	575	+58	366(65)	926	−69
		CR = 1.21			CR = 1.13	
			$CR_d = 1.65$			

*In the "Number of Subjects" columns, the figures not enclosed in parentheses indicate the number of different subjects in a given classification. For those series in which there were two sessions, the figure in parentheses refers to the number of subjects who were in a given classification for one session only. For example, in Series S6, twelve subjects (figure in parentheses) were expansive for one session, but compressive in the other session. When these 12 are subtracted from the total of 14 expansives, two remain who were expansive for both sessions. Similarly, among the compressives there are 18 who were compressive for both sessions (30 minus 12).

The total number of subjects tested in a series is equal to the number of subjects in the expansive and compressive groups minus the smaller of the two figures given in parentheses. (In Series S6, 14+30=44; 44−12=32, total number of subjects.) Differences in the figures in parentheses in the two groups are due to the fact that a few subjects participated in one session only.

ference between the two groups. In nine of the series the application of the expansion-compression measure produced differences in the expected direction, while in the others there was either a reversal of the trend or no difference at all.

Although a total of 1,505 ESP runs were made in these series, the expansion-compression analysis was made on a total of 1,501 runs, while only 1,427 runs were represented in the interest inventory analysis (cf. Tables 1, 2, 3). The main reason for the difference in these figures is that some subjects on whom expansion-compression ratings were available did not fill out the interest inventory. There are also a few cases in which a subject filled out the inventory but did not turn in his drawings. Thus, in the analysis combining both personality measures, we can use only the results of subjects on whom both measures were secured. In this classification there were 515 subjects, who made a total of 1,421 ESP card runs.

In order to compare the results of the combination of measures with the results of each measure alone, it is necessary to present the results of each measure alone when applied only to those subjects on whom both measures are available. When the ESP results of the 515 subjects mentioned above are separated by the interest inventory measure, the following figures are obtained:

Midrange Group			Extreme Group			Diff. Between
Runs	Dev.	Av. Score	Runs	Dev.	Av. Score	Av. Scores
709	+105	5.15	712	−138	4.89	.34

The CR of the difference between the average scores of the two groups is 3.22, which is close to the figure of 3.19 obtained when all subjects who had taken the inventory were considered.

The figures obtained when the results of the 515 subjects are separated by the expansion-compression criterion are given below:

Expansive Group			Compressive Group			Diff. Between
Runs	Dev.	Av. Score	Runs	Dev.	Av. Score	Av. Scores
541	+55	5.10	880	−88	4.90	.20

The CR of the difference between the average scores of the expansive and compressive groups is 1.85, which is somewhat higher than the CR of the difference of 1.65 obtained when the results of all expansive and compressive subjects were considered in Table 3.

Combination of Personality Measures

First Section. Those subjects in the First Section who received both expansive and midrange ratings made a total of 166 ESP runs, with a deviation of +68. (See Table 4.) The CR for this group is 2.64. On the other hand, the subjects in the First Section who received both compressive and extreme ratings gave a total of 246 runs with a deviation of −96. The CR for this group alone is 3.06. The difference between the means of these two groups is significant, with a CR of the difference of 3.98. The other two groups of subjects—the expansive extreme and the compressive midrange groups —scored virtually at the chance level.

The results obtained when the interest inventory and the expansion-compression measures are considered separately may be more easily compared with the results obtained from a combination of the two if we compare the average run scores for the various groupings in the First Section. These averages for the interest inventory measure, for the expansion-compression measure, and for the two measures combined are given below, together with the differences between the averages.

Group	Average Score	Group	Average Score	Group	Average Score
Midrange	5.15	Expansive	5.13	Expansive Midrange	5.41
Extreme	4.67	Compressive	4.76	Compressive Extreme	4.61
Difference	.48		.37		.80

From these figures the increased effectiveness obtained by combining the two measures is even more evident. The difference between the averages is substantially greater for the combination than for either of the two measures used alone.

Second Section. When the combination of measures was applied to the data of the Second Section, the expansive midrange subjects made a total of 127 runs with a deviation of +14. The subjects who were compressive and in the extreme group gave a total of 218 runs with a deviation of −15. Neither of these figures is significant, nor is the difference between the two (CR of the diff. = .80). Moreover, as may be seen from Table 4, the highest result is not given by the expansive midrange group as it was in the First Section but by the compressive midrange group. The

Table 4

ESP Results for the Four Groups of Subjects Resulting from a Combination of Interest Inventory and Expansion-Compression Measures

First Section

Series	1. Expansive Midrange Group			2. Expansive Extreme Group			3. Compressive Midrange Group			4. Compressive Extreme Group		
	Number of Subjects*	Runs	Dev.	Number of Subjects*	Runs	Dev.	Number of Subjects*	Runs	Dev.	Number of Subjects*	Runs	Dev.
S1	13(7)	38	+22	8(4)	24	−14	11(7)	30	0	13(4)	44	−15
S2	11(3)	38	+ 7	12(7)	34	−11	8(4)	24	− 8	11(7)	30	− 8
S3	9(6)	24	+11	5(3)	14	+11	10(7)	26	+ 2	10(3)	34	−11
S4 Ma	6	12	+11	7	14	− 5	11	22	− 2	10	20	+ 8
S4 Sm	5	10	− 1	5	10	− 8	9	18	+15	8	16	−11
S5	4	8	+ 3	3	6	+ 6	14	28	− 1	14	28	− 2
S6	7(6)	16	0	6(5)	14	−12	14(6)	44	−15	14(5)	46	−31
M1	5	20	+15	3	12	+ 4	5	20	− 3	7	28	−26
Total	60(22)	166	+68	49(19)	128	−29	82(24)	212	−12	87(19)	246	−96
		CR = 2.64									CR = 3.06	
	CR of the diff. (Group 1 − Group 4) = 3.98											

Second Section

Series	1. Expansive Midrange Group			2. Expansive Extreme Group			3. Compressive Midrange Group			4. Compressive Extreme Group		
	Number of Subjects*	Runs	Dev.	Number of Subjects*	Runs	Dev.	Number of Subjects*	Runs	Dev.	Number of Subjects*	Runs	Dev.
S7	6	12	+ 6	6	12	+ 4	14	28	+ 7	14	28	+15
S8	4	8	− 4	5	10	+ 2	10	20	+ 2	10	20	− 6
R6	5	10	+ 9	6	12	0	14	28	− 3	12	24	− 2
R7	9	9	+ 4	8	8	− 3	8	8	− 9	10	10	− 1
R8-A	8(1)	30	−11	10(6)	28	+ 5	10(1)	38	+12	15(7)	46	− 5
R8-B	11(7)	30	+ 6	7(4)	20	+10	16(8)	48	+ 7	17(4)	60	−15
H2	7	14	− 4	7	14	− 5	7	14	+ 9	7	14	−10
H4	7	14	+ 8	8	16	−11	10	20	+10	8	16	+ 9
Total	57(8)	127	+14	57(10)	120	+ 2	89(9)	204	+35	93(11)	218	−15
		CR = .62									CR = .51	
	CR of the diff. (Group 1 − Group 4) = .80											

Both Sections Pooled

Series	1. Expansive Midrange Group			2. Expansive Extreme Group			3. Compressive Midrange Group			4. Compressive Extreme Group		
	Number of Subjects*	Runs	Dev.	Number of Subjects*	Runs	Dev.	Number of Subjects*	Runs	Dev.	Number of Subjects*	Runs	Dev.
Grand Total	117(30)	293	+82	106(29)	248	−27	171(33)	416	+23	180(30)	464	−111
		CR = 2.40									CR = 2.58	
	CR of the diff. (Group 1 − Group 4) = 3.48											

*For the explanation of the figures in parentheses in the "Number of Subjects" columns, see footnote to Table 3. In Table 4 the total number of subjects tested in any series (or group of series) is equal to the sum of the number of subjects listed in the four columns minus the sum of the figures in parentheses for *either* the expansive *or* compressive subjects. Where the figures in parentheses for these latter groups are not equal, the smaller figure should be taken. Example, total number of subjects tested = 117 + 106 + 171 + 180 − 30 − 29 = 515.

average run score for the latter is 5.17, while that for the expansive midrange group is only 5.11.

The average run scores for the various groups obtained by the use of the two measures singly and together are given below for ease of comparison.

Group	Average Score	Group	Average Score	Group	Average Score
Midrange	5.15	Expansive	5.06	Expansive Midrange	5.11
Extreme	4.96	Compressive	4.95	Compressive Extreme	4.93
Difference	.19		.11		.18

Although the use of each measure alone resulted in a small difference in the expected direction between the averages, the combination of the two measures did not show the increased effectiveness found in the First Section. The difference obtained for the combination is slightly smaller than that obtained from the interest inventory measure alone.

Both Sections Pooled. When the results of both sections of the data are pooled, we find that the expansive midrange group of subjects gave a total of 293 ESP runs with a positive deviation of 82. The CR for this group alone is 2.40. The 464 runs of the compressive extreme group, on the other hand, gave a deviation of −111 with a CR of 2.58. The difference between the ESP scores of the two groups is statistically significant, with a CR of 3.48. The other two groups of subjects (the expansive extreme and the compressive midrange) gave insignificant results as may be seen from Table 4.

The comparison of the effectiveness of the two measures applied singly and in combination is more easily made from the average run scores of the various groups for the entire data. These figures are given below and are also graphically represented in Figure 1.

Group	Average Score	Group	Average Score	Group	Average Score
Midrange	5.15	Expansive	5.10	Expansive Midrange	5.28
Extreme	4.81	Compressive	4.90	Compressive Extreme	4.76
Difference	.34		.20		.52

The difference in the average scores shown in the column on the right is almost as large as the differences found in the other two columns added together.

In the pooled results of all sixteen series, then, the combination of the two personality measures gave a larger difference between groups than was given by either measure alone, and this difference was statistically significant (CR of the diff. = 3.48).

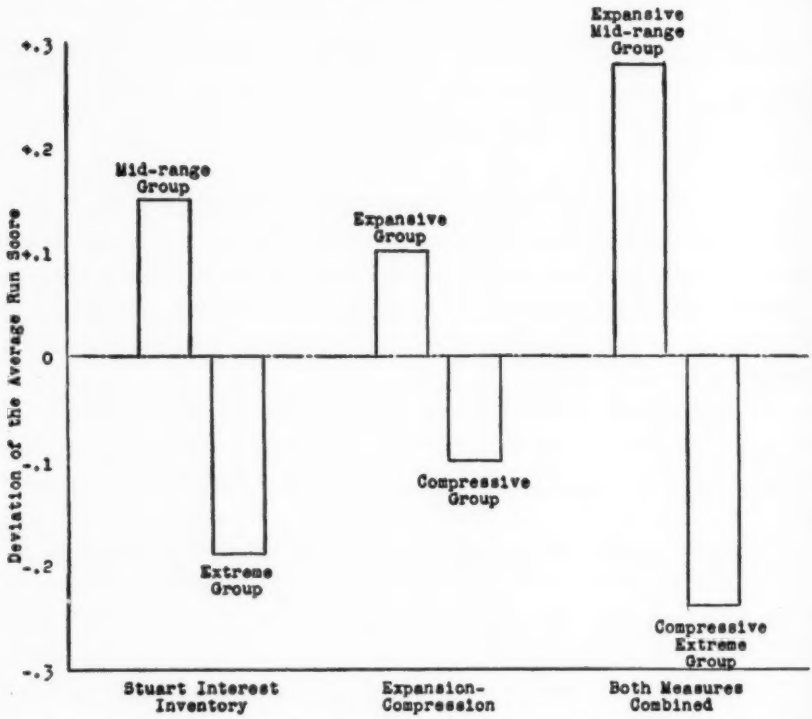

FIG. 1. Deviations of the average run scores for the groups of subjects obtained when each personality measure was used alone and when the two were combined.

DISCUSSION

For purposes of discussion the results described above may be summarized briefly as follows: A total of 1,421 ESP runs gave an insignificant deviation of −33 when analyzed for evidence of clairvoyance. When these data were broken down by the expansion-compression measure, however, a suggestive but insignificant difference was found between the average ESP scores of expansive and compressive subjects (P = .064). Application of the Stuart Interest Inventory measure to these same data resulted in a signifi-

cant difference between the average ESP scores of the midrange and extreme subjects (P = .0012). When the data were further analyzed in terms of a combination of both the interest inventory and the expansion-compression measures, two groups of subjects (expansive midrange and compressive extreme) were found whose average ESP scores were significantly different (P = .0004). A comparison of the differences in average ESP scores for the two personality measures considered separately and in combination shows that the combination resulted in a larger difference than was obtained by using either measure alone. (See Figure 1.)

Some supplementary findings obtained from further analyses of the data will add to the picture of the results. In considering the two groups of subjects in whom our main interest was centered—the expansive midrange and the compressive extreme groups—it is proper to inquire into the consistency of their ESP results. An examination of the data reveals that 50% of the expansive midrange subjects gave a positive deviation on their ESP test, 8% gave a zero deviation, while 42% gave a negative deviation. For the compressive extreme group, the comparable figures are: positive, 35%; zero, 16%; negative, 49%. These figures are based on the total deviation obtained by each subject in a given session.

The relationship between the personality measures and the direction of the subjects' deviations in the ESP test (that is, positive, zero, or negative) may be further evaluated by the use of a contingency table compiled from the figures from which the above percentages were obtained. The number of subjects in the expansive midrange group who obtained positive deviations (regardless of the magnitude) is found and is entered in the upper left-hand cell of the contingency table given below. Similarly, a count is made of the number of subjects whose total deviation per session was either negative or zero. Following a practice we had adopted earlier (2), we arbitrarily combined the number of subjects in the negative- and zero-scoring categories and entered this figure in the upper right cell of the table. In the two lower cells of the table are given the comparable figures for the compressive extreme groups of subjects. The following table was obtained:

	ESP Score		
	+	O, —	Total
Expansive Midrange Subjects	73	73	146
Compressive Extreme Subjects	80	150	230
Total	153	223	376

The chi square derived from this table is 8.57, which, with one degree of freedom, has a probability of approximately .003. This result indicates that the sign of the ESP score is related significantly to the personality ratings of the subjects.

A similar analysis was also carried out using individual run scores instead of the total deviation obtained by each subject in each session. It was found that the expansive midrange subjects gave more positive-scoring runs than negative-scoring ones and that the reverse effect was present in the compressive extreme group. A chi-square analysis of the contingency table made up on the basis of the number of run scores having positive, zero, or negative deviations for the two groups of subjects gave a chi square of 12.65. With one degree of freedom, this chi square has a probability of approximately .0004. This analysis is not independent of the one just preceding, but it is a useful way of looking at the results in that it shows the degree of run consistency.

These various analyses show that the significant difference between expansive midrange and compressive extreme subjects for all series combined was not due to a few outstanding subjects nor to a few outstanding runs. A large number of subjects in both groups contributed to the effect.

Analyses of the results of the two groups of subjects expected to score near the chance level revealed that their scores followed the predicted trend. Chi squares based on contingency tables similar to those given above for the other groups were insignificant for the compressive midrange and expansive extreme subjects.

Throughout the various analyses it has been noted that the significance found was contributed primarily by the series in the First Section. The Second Section results usually followed the same

trend but were never independently significant. It is hard to formulate reasonable explanations for the failure of this section. Some of the possible variables involved in these series were discussed in the previous paper (2) when the question was raised as to the failure of the interest inventory measure to reveal significance in this section of the data. On the basis of the various analyses made, it seems safe at least to conclude that there was very little, if any, ESP ability operating in these series.

The two aspects of personality dealt with in this study differ in a fundamental respect. The interest inventory score appears to reflect a more enduring state of the personality, while the expansion-compression measure seems to reflect the subject's mood of the moment. Unpublished data on the inventory suggest that, when subjects take the inventory twice with several months intervening, there is relatively little tendency for them to change in their interests. On the other hand, it can be seen from the present data that some subjects even change from expansive to compressive, or vice versa, from one session to the next. It has been found in other studies (3, 7) that when a given subject does change in his expansion-compression rating, his ESP scores tend to change in the predicted direction also. The expansion-compression criterion then should be of use not only in separating high-scoring subjects from low-scoring ones in general, but also in separating the high- and low-scoring runs of a given subject. Only a measure of a transient mood or state of the person would be capable of doing the latter. The results of the present study, therefore, suggest the importance of including measures of both the relatively stable and the relatively transient states of personality in any composite test of personality devised for the purpose of separating high-scoring subjects in ESP tests from low-scoring ones.

It must be kept in mind that in this report the combination of measures has been applied to data obtained from a specific type of ESP test—a clairvoyance card test—and that, with one exception (Series M1), the tests were given to groups of subjects. Whether or not the same combination of measures will be effective in dealing with data obtained in other types of ESP tests, under different conditions, and by other experimenters is a problem which will have to be answered by additional research.

In the past, several other researches have involved combinations of personality measures, and the results were similar to those of this report; that is, a larger difference was obtained from a combination of measures than was given by any one measure alone. Schmeidler combined ratings on subjects' attitude toward the theoretical possibility of ESP with several ratings from the Rorschach records of the subjects (6). The expansion-compression measure was used in conjunction with ratings based on the Maslow Security-Insecurity Test in an experiment reported by Smith and Humphrey (7). It is reasonable to hope that other such combinations will greatly increase our control over the phenomenon of ESP. From the vast experimental literature, we have clues pointing to a myriad of factors influencing ESP performance. We can hardly expect one or two personality measures alone to give us a perfect separation of high- and low-scoring subjects. At the present time we can hope to get a significant degree of separation only on the basis of a very large number of test data. In time, however, as we learn more about the factors influencing ESP performance and as we find means of measuring these factors, we should more closely approach the ideal of precise prediction of ESP scoring levels.

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MINOR ARTICLES AND NOTES

CASE BOOKS OF THE LAST QUARTER CENTURY

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The major research in parapsychology of the last twenty-five years has been experimental. Although human experiences which seem to be parapsychological have been occurring in an unending stream through these years, they have been disregarded in the main, since they are open, as evidence, to all the criticisms that can be leveled at human testimony. Early English workers made voluminous collections of reports of spontaneous psychic experiences which they screened and verified with care. The aim was so to validate the reports that they could be taken as proof of the occurrence of psi phenomena. Time did not justify this effort, however, and the tendency has been to ignore the case material and to rely for proof entirely upon the results of experimentation.

A new attitude toward cases, however, has developed in recent years. It was expressed editorially in this Journal (7) recently, and the value that might be derived from case material by experimentalists was outlined. It was pointed out that one of the present needs of the experimenter in parapsychology is to get new leads and suggestions for the planning of future research. There is ample proof now of the existence of the psi capacity, but too little is known even yet of its nature and of the conditions under which the ability is best expressed. It is time, then, for a survey of the more natural sources, such as spontaneous cases, to discover what new suggestions they may offer. Overlooked or forgotten in the volumes of material of this nature may well lie clues that will prove of great value and which could help to narrow the gap between natural and laboratory situations.

But even though little attention has been given cases in the last twenty-five years, a few publications dealing with them have appeared. A brief survey of these and a word about the objectives of the various authors may be useful in calling the material involved to

the attention of those who are interested in looking it over from the viewpoint referred to above.

In 1928 Dr. W. F. Prince's book, *Noted Witnesses for Psychic Occurrences* (**6**),came out. It contained a collection of cases gleaned from various published sources. The basis of selection was not the nature of the case reported, but the standing and prominence of the person who experienced it. Dr. Prince's idea was that although a "noted" witness is not necessarily more creditable than an obscure one, it is generally easier to trust the former than the latter. His "noted" witnesses are grouped into ten chapters, ranging from men of science through the arts, professions, and business world to travelers, magicians, and people of title. Since the subject matter is organized in this way, it is obvious that the objective was to validate the anecdotes by authority rather than to attempt to classify or analyze them on the basis of their intrinsic nature. For this reason, it would be necessary to reorganize the material to make it readily usable for parapsychologists.

Dr. Prince published another and larger collection of cases, *Human Experiences* (**5**), in 1931. His objective here was to survey a limited section of the population, chosen on the same assumption as that of the earlier book, that prominent and educated people would be expected to give more reliable testimony than the common man. Accordingly he sent a questionnaire regarding psychic experiences to ten thousand people whose names were listed in *Who's Who.* Some two hundred of the accounts of cases which he received in reply are reported in the book under the chapter headings: "Dreams," "Apparitions," "Monitions and Premonitions." A supplementary bulletin was issued also, in which were cases listed as spontaneous telepathy and experimental telepathy. The discussion is not about the content of the case material, but about the questionnaire method itself and inferences drawn from a study of the distribution of cases among the population which was queried.

The next two publications, H. F. Saltmarsh's article, "Report on Cases of Apparent Precognition," 1934, (**9**), and Dame Edith Lyttelton's *Some Cases of Prediction,* 1937, (**4**), both appeared before any experimental findings on precognition had been published. But the question as to the possibility of a human faculty of precog-

nition had long been raised by experiences of apparent premonition which have been reported as far back as historical records go.

Saltmarsh's article represents an attempt to come to grips with the question of precognition on the strength of the case evidence. First he presented a number of reports of such experiences. These were drawn from those published earlier in the *Journal* and *Proceedings of the Society for Psychical Research.* After careful screening he decided that there is a residue of cases which cannot be explained by any other hypothesis than by that of precognition. In his paper he then offers a working hypothesis of precognition. The article is therefore not a case study in reality; the cases cited in it are only incidental to the problem of formulating a hypothesis to explain precognition.

In 1934 a series of radio broadcasts was undertaken in England on the topic, "Inquiry into the Unknown," with various persons taking part, including Professor Broad, Dame Edith Lyttelton, Sir Ernest Bennett, Sir Oliver Lodge, and others. Dame Edith Lyttelton's little volume, *Some Cases of Prediction,* is made up of sixteen cases culled from the large number of replies which she got from her part in the program. The objective of her publication as stated is to "stimulate an interest in the scientific investigation of supernormal faculties of the mind . . . with particular emphasis on precognition."

There next appeared in 1938 a small case book by W. H. Salter of the English S.P.R., *Ghosts and Apparitions* (8), which was published with the objective of calling attention to the large amount of well attested data under this heading in the older literature. The author hoped, by making such material more accessible, to stimulate theorizing concerning it. The cases selected are divided into the following categories: dreams, apparitions seen by percipients when awake, hauntings, and poltergeists. The author discussed the theory that telepathy might be the explanation of the occurrences, but decided that it could not explain them all. He also discussed the old arguments and evaluated the various possibilities of mistakes and errors in the evidence, and ended with the conclusion that all of this must mean something, but that much more study and experimentation are needed before it could rightly be said what it means.

Sir Ernest Bennett's volume, *Apparitions and Haunted Houses*

(3), appeared in 1939. Its purpose was to present a survey of the evidence on this topic furnished by reliable witnesses and to see what hypotheses would be necessary to meet the facts. He took part in the same radio broadcast series mentioned before in which Dame Lyttelton also participated. Fifty of the letters he received are included in this volume, the rest of the cases presented being drawn presumably from the old source cases previously printed by the S.P.R. However, references are not given and most of the dates incidentally given are from the last century, so it is not easy to tell which of the cases are new and which are old. One hundred and four cases are reported, and they are divided under the following headings: "Recognized Apparitions," "Unidentified Apparitions," "Apparitions of the Living, of Animals, and of Inanimate Objects."

The cases in A. T. Baird's book, *One Hundred Cases for Survival after Death,* 1944 (2), are drawn from various sources, but the majority of them come from earlier psychic research publications. Although in the light of present knowledge, many cases presented in this volume need not be interpreted as evidence of survival, they can be of suggestive value for researchers. The same remark is applicable to this author's second volume, *Casebook for Survival* (1).

The most recent book of cases is *"The Mystery of Dreams"* by William Oliver Stevens (10) which came out this year. The instances of psychic dreams reported in it have been drawn from different sources—most, but not all of them, from previously published accounts. The selection, the author states, was made on the basis of variety and in the interest of readability since the aim was to stimulate popular rather than scientific interest. The selection of material is careful enough, however, to be worthy of attention by parapsychologists.

This concludes the list of the more serious publications of the last quarter century which deal with spontaneous psi experiences. The fact that the list is a short one and that much of the material included in these books is drawn from the old collections reflects the lowered emphasis on case studies in the parapsychological work of the last few decades. Nevertheless, there is new material in most of the publications mentioned. This fact should help to make these

volumes of interest to the student of parapsychology who takes the new approach to spontaneous psychic experiences.

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A CASE OF APPARENT ESP IN A DOG

By Cecil French

[The following report of apparent extrasensory perception in a dog is of special interest in connection with the article, "Direct Influence of a Person upon the Behavior of Animals," by W. Bechterev which appeared in the September Journal. The author is a doctor of veterinary science.—Ed.]

The following is a truthful account of an outstanding event in my experience with dogs, and it will take a very wise person to offer an explanation thereof. There must be a few individuals alive today who will recall the exhibition of this extraordinary creature.

From 1894 until 1914 I practiced as a specialist on canine disorders in the city of Washington, D. C. Into my office one day walked a gentleman with a Collie bitch named "Miss Dozie," seeking advice on the treatment of a minor trouble. After we had gotten through with the object in view, the owner, whose name I have unfortunately forgotten, remarked, "I suppose you will not believe me when I tell you that that dog is a mind-reader. If you doubt my word, I will let you make the test yourself." He informed me further that he had been asked to bring this remarkable animal to Washington from his home in Chicago at the request of the National Republican Committee for the entertainment of President Theodore Roosevelt at the White House.

He then proceeded with the demonstration by asking me to hold up as many fingers as I wished behind my back, which I did. Turning to the dog, he asked, "How many fingers does the gentleman hold up?" Miss Dozie barked correctly. I repeated the action three or four times, varying from one to ten fingers, and on each occasion she barked correctly. There was no mirror in the room, and no possibility of any shadow to have conveyed information to the owner whereby he might have signaled to the dog. It was an astounding experience, and the owner, noticing my bewilderment, remarked, "You think there is some collusion between Miss Dozie and myself. Therefore I am going to let you make the test entirely on your own.

I will leave the office and remain outside on the street while you carry out the test, and I suggest that you put her in the next room, close the door, and call to her through the latter (which had a glass window) so that there will not be the possibility of her observing any action on your part." He left the office and I did as suggested, closing the door and calling to Miss Dozie through the latter. Again she barked correctly on each of two or three tests.

At that stage I discontinued the test and invited her back into the front office whence I intimated she should go outside and join her master. As we emerged from the front door he asked her, "How many people are on the street, Miss Dozie?" She barked three times. He followed that directly by the question, "How many white?" at which she barked twice, indicating her master and myself. That was followed quickly by a third question, "How many colored?" at which she barked once, and on looking down the street I perceived a negro coming toward us. There was no one else in sight at the moment.

That ended the entertaining test which has ever since left me wondering at this most marvelous exhibition of psychophysics on the part of that dog, and I am still awaiting an explanation.

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GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

CALL v.: To attempt to identify by ESP a stimulus object or a mental state of an agent.

CALL n.: The response described above; also the resulting selection.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI-SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards, five of each suit.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibility of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single trials, p the probability of success per trial, and q the probability of failure. (For ESP cards, SD $= 2\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ_s is the SD of a single run and R_1 and R_2 are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. Common criteria are: a probability value of .01 or less, or a deviation in the expected direction such that the critical ratio is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.

OBJECTIVES

We hope to encourage a greater scientific interest in parapsychology —that is, in the study of those phenomena attributable to human personality which in some degree transcend physical explanation.

We hope that eventually all problems belonging in the field of parapsychology will be brought under careful consideration and solved as completely as the nature of the problem and the methods of science will permit, without fear of or favor toward prevailing views on the question at issue.

We hope that all those who share our interest in the problems of parapsychology and who wish to see them solved by the sure and final methods of science will join with us in whatever way is available to them. There are few who can contribute to the actual researches as experimenters and few who can contribute the means by which others may experiment. Many, however, can serve in other important ways, for the needs are great. Some can report cases of interest to those who are doing the research; others can write or lecture and interpret our reports to a wider audience than would normally be reached. Others may have suggestions, criticisms, or interpretative comments to offer. Words of encouragement alone are worth much to the explorer. The field is big enough to allow a great diversity of activity and wide differences in viewpoint to exist without friction; it is a field full of the greatest challenge for constructive endeavor.

We hope that adequate laboratories and fellowship facilities will be created to enable willing and qualified experimenters to devote their full energies to a broad program of much-needed research in parapsychology.

We hope there will be an increased appreciation of the great philosophical significance of the findings in parapsychology and the part they must eventually play in the cultural history of mankind.

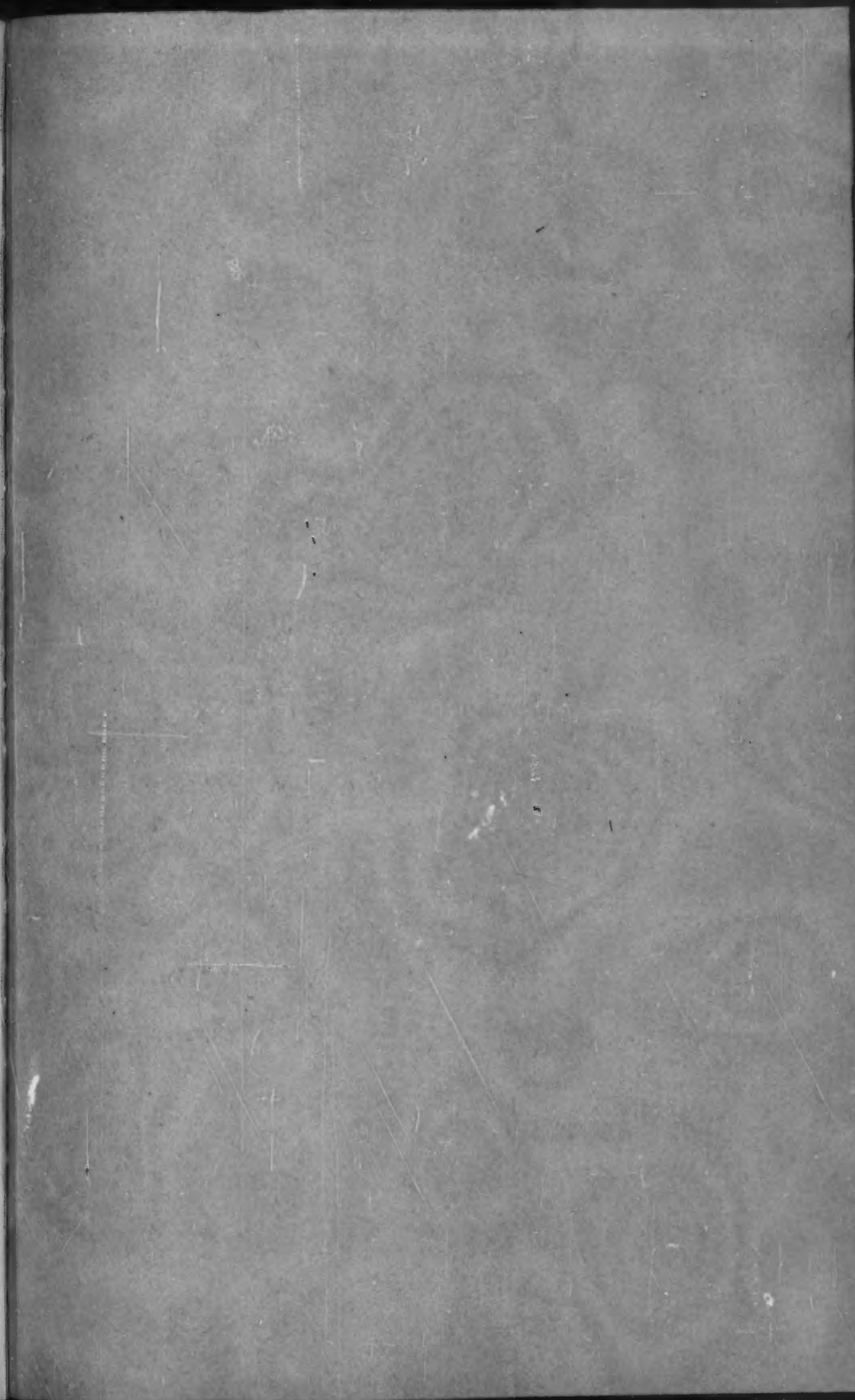
We hope that parapsychologists will direct their efforts primarily toward the search for demonstrable truth and to the exposure of error only in so far as it serves that purpose.

THE EDITORS.

A THEORY OF OPPOSING PREJUDICE

We must not altogether blame men of science for feeling this prejudice [against telepathy]. It is a kind of self-defense. In attempting to understand the universe what may be called an orthodox schematization grows up, and forms a framework giving guidance and coherence to the advance of science. This is useful and even indispensable, and danger only arises when the scheme becomes a system of dogma which is master instead of slave. But the usefulness of such a framework is partly that it prevents waste of energy in pursuing wild ideas, in running after every hare that may be started. It thus becomes natural for orthodox science to view with suspicion and even alarm anything which threatens to disturb the framework in the slightest degree, or—to use another metaphor —anything which seems to open up an alley regarded as finally closed. Now telepathy does threaten to reopen questions which to some have seemed settled, and it must therefore expect to be rejected or ignored by many, and to have its credentials very closely examined even by the most open-minded representatives of orthodox science before they admit that there is so much as a *prima facie* case for investigation.

Here again our man in the street with his too facile acceptance does us harm, for the standard of evidence which satisfied him does not, and ought not to, satisfy the scientific man, and the latter perceives this and makes it an excuse for looking no further.

Well, for us who are convinced that there is at least a case for investigation, it is clear that the proper course is first to keep the standard of evidence high; secondly, to aim at increasing the amount till it is impossible for anyone to overlook it; and thirdly, to endeavor to systematize it, show how it works and what it points to. This last is of course the essential aim of the investigation itself, and plenty of good evidence is an essential prerequisite.

—Eleanor Mildred Sidgwick, from *Mrs. Henry Sidgwick, A Memoir* by Ethel Sidgwick, 1938